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RELIGION AND THE UNKNOWN.

By Richard A. Proctor.

Behold God is great and *we know him not*. . . . Great things doeth he which *we cannot understand*. . . . Touching the Almighty *we cannot find him out*.—*Elihu* (*the approved of God*) *in the Book of Job*, chap. xxxvii.

Who hath known the mind of the Lord?—*Paul* (*to the Romans*), chap. xi.

IN a kindly notice of my essays on "The Unknowable" in Knowledge, which I saw recently in *Watts's Literary Guide*, the remark is made that though I "object to the term 'agnostic' as inadequately expressing my views," I am "in sympathy with those who are recognised as belonging to the agnostic school of thought." I am not sure that I have ever expressed any objection against the term "agnostic," though I admit that no such term can quite adequately express the views of a school; but emphatically I am in sympathy with the school called, for convenience of reference, Agnostic. I wish here briefly to show why; and more particularly to point out that in all ages and among all races true and reverent religion has been necessarily agnostic.* I have expressed this thought in Knowledge in the words, "A God understood is no God at all." I might equally have said, with the same underlying meaning, "A religion which may be presented in terms of the known is *no* religion."

So soon as man rose above the merely animal condition he began to be moved by the unknown and by the unknowable, though as yet he had no means of distinguishing one from the other. He showed the influence of the unknown upon his emotions by the constant effort to picture it as known and understood. In this effort religion had its beginning and all developments of religion had their origin. What was nearest and simplest moved man first. The river and the forest, the mountain and the sea, rain, wind, and storm, representing unknown and for the child-man unknowable power, were early regarded as known deities, only mysterious in their attributes as deities, in their nature and qualities, their will and purpose. Men recognised in the very unknowableness of natural phenomena the known fact that in these, at any rate, they might worship a power outside themselves—in other words, Deity. But as time went on, as the life of the huntsman and the fisherman gave place to pastoral life—nomadic first, and then more settled—and pastoral to agricultural life, the forces with which man had just come into more obvious contact, the forces belonging to *the earth*, lost in dignity and impressiveness in presence of the forces which seemed present in *the heavens*. The sun in his glory and his obvious might, the moon walking in splendour, the planets as they pursued

> Their obvious course, now high, now low, then hid,
> Progressive, retrograde, and standing still,

began to be recognised as the really mysterious powers of the universe. These now seemed appropriate objects of worship and adoration, because their movements and their influences seemed inexplicable, and being thus apparently unknowable might be regarded as assuredly divine. Men now, as before, in what seemed certainly unknowable, found certainly known divinity. And because this way of viewing the heavenly orbs necessarily lasted for many centuries in every race, for thousands, nay, tens of thousands of years among mankind, the worship of these mysterious divinities—mysterious, yet in the very assurance of their mystery regarded as demonstrably divine, affected all races which passed beyond a certain stage, to the very heart's core. No matter what advance might subsequently be made, no race has ever *as* a race, shaken off that long-established religion which had the dome of the heavens for its temple, the visible orbs of heaven for its deities. In the temples of to-day, among Jews, Mohammedans, Christians, and Buddhists—anxiously though the teachers of these religious bodies have endeavoured to get rid of nature-worship—the ceremonial observances relating to the heavenly bodies remain. Our temples now bear to the great temple of the sky the same sort of relation which the Lady Chapels of the Middle Ages bore to the great cathedral beneath whose roof they found a place.

In the worship of the heavenly bodies, there was the worship of the unknown, and, as was thought, the unknowable. So soon as the movements of these bodies were known and could be predicted they ceased to retain their position as deities, except for the ignorant, who, however, were the many, as they have ever been. Here began, as the ignorant supposed, a contest between science and religion. Science attacked religion, as they understood religion, in the very fact that science endeavoured to explain what, so long as it was believed to be inexplicable, men could worship. Science destroyed the significance and value of their sacrificial system so soon as science learned to predict that at such and such a time the sun would rise again, the moon which had waned would refill her orb, the sun as God of the year would ascend, after his spring passover, to the full glory of the midsummer heavens. Men cannot worship the vernal equinox or the sun's right ascension, as they had worshipped the sun-god at his passover, or after he had ascended to the mid heaven. The "Nautical Almanac" cannot replace the greater and the lesser prophets; nor can tabular announcements of sunrise and sunset, of the moon's quarters, or of the equinoxes and the solstices, take the place of the morning and evening sacrifices, the "blowing of the trumpet in the new moons," the Feasts of the Passover and of Tabernacles, or the Fast of the Atonement for the waning of the sun-god. Observation has displaced observance, study has displaced reverence, so far as the religion of the heavenly orbs has been concerned. There was in a sense a long-lasting conflict between science and religion, as knowledge displaced nescience, and men found that what they had deemed an inexplicable mystery—the veritable unknowable—could be interpreted and understood. But it was because the worship of the heavenly bodies was no true religion that conflict arose with growing science, and that grown science slew the religion which was based on false conceptions.

* With this volume of Knowledge the papers and notes on the relations between religion and science which have appeared in Knowledge from the time of its issue as a monthly magazine, will be brought to a conclusion. They will probably be collected soon in book form.

So long as religion attempts to deal with the unknown as the absolute unknowable, and fixes its domain within the mistaken limit thus drawn, conflicts must ever be renewed with growing science, and the event of such conflicts must be ever the same. When religion ceases to proclaim that "Deity is here, or It is there," and admits that the force lying at the back of all phenomena, Absolutely Unknowable Power, can alone be accepted as the truly divine mystery of the universe, then and then only the advance of science will become (as it should be) a matter of no moment so far as religion is concerned. When men pictured Hell as the lower story, Heaven as the top story, of a world whereof the middle story was the earth, science, in showing the earth to be a mere point in space, was shaking the very foundations of religion. But that was because religion was based on falsely-assumed knowledge. If science is shaking the foundations of religion now, it can only be for a similar reason. True religion has no more occasion to fear science than the infinite has occasion to fear astronomy.

THE STAR OF BETHLEHEM.

By RICHARD A. PROCTOR.

A EW things perplex me more than the fanciful way in which the general public take up some matters of scientific interest and neglect others. Although my real work lies in scientific study rather than in the exposition of scientific matters, I am considerably interested in studying the manner of thought of those whose avocations are for the most part unscientific. And repeatedly I have been surprised, not to say disappointed, by the lax attention of the public in regard to matters which to the student of science appear intensely interesting, and the earnestness, almost enthusiasm, with which the same public will consider matters relating, indeed, to science, but in which the student of science will hardly consent to take any interest at all, so trivial are they, or uncertain, or unfruitful.

Amongst the matters about which I have received the greatest number of inquiries I may mention specially what the unscientific choose to call the Star of Bethlehem. I am not referring now to the celestial appearance recorded as seen in the East at the time of the Nativity—a subject already dealt with fully in these columns. I am considering a certain new star seen in the year 1572, which persons ignorant of astronomy insist on calling, without a shadow of real reason, the Star of Bethlehem. I know not to whom the honour of inventing this ridiculous title for the star should be assigned. Probably one those semi-religious, but wholly ignorant fanatics, who look to the heavens for signs and portents relating to the affairs of men, and imagine that the fires which are presently (they assert) to destroy this sinful earth will come straight down on us from the celestial canopy over our heads, conceived the brilliant idea. But, whoever first thought of it, the idea has been received with enthusiasm by kindred minds; and such minds being much more numerous than minds more reasonable, the notion is found knocking about all over the world, in company with an amount of ignorance about even the elements of astronomy which is truly distressing in its bewildering completeness.

The latest development of the preposterous notion that a remote variable sun in Cassiopeia, probably at least a million times further from us than our own sun, is the same orb which led the astrologers of Chaldea first to Jerusalem, and then, after disappearing for a time, from Jerusalem to Bethlehem (an eight miles' walk) has now been announced. A "professor"—bless us!—of Hartford, Ky., has actually seen the new star in Cassiopeia, close by the point overhead, a little towards the north. Astronomers will be more startled by the position assigned to Cassiopeia than by the discovery of a new star in that well-known constellation. Any one could look at Cassiopeia and imagine—being ignorant—that some perfectly well-known star in that constellation is a new one. In fact, this Kentucky "professor," by remarking that there are five bright stars in Cassiopeia, "and two or three others," shows clearly enough what oddly incorrect ideas he has about the array of stars in that compact but tolerably rich constellation. What is really stupendous in his achievement, is not his seeing a new star in Cassiopeia, but his seeing Cassiopeia in a new place. Could Cassiopeia be seen at night in the middle of May towards the zenith, most assuredly the discovery would merit most serious attention. For such a portent would imply much more serious trouble than the appearance of a whole legion of new stars. An old constellation (and Cassiopeia is very old) appearing in a new place, would imply very serious terrestrial disturbance. If the astronomers we pay to keep track of time by close survey of the stars—our Government astronomers, I mean—had to report Cassiopeia now as in the zenith, at any time between nightfall and morning twilight—matters would look unpleasant indeed. Old earth would have to wobble from her true position in fearful fashion to bring about such a portentous phenomenon as that; for, as a mere matter of fact, Cassiopeia ought at midnight in May to be nearer the horizon than the point overhead. It has been suggested that the miracle of the standing still of the sun can only be explained, for modern science, by the staying of the earth's rotation during a certain interval, the earth resuming her steady spin when the Jews had done their fighting; to which the sceptical crew who are always suggesting troublesome doubts, have objected that if the earth stopped rotating, every earthly thing would have been destroyed by the shock, and consumed immediately afterwards by the tremendous heat instantly generated. But this would be nothing compared with the significance of the Kentuckian professor's miracle. For this would require not merely such a trifle as that the earth's spin should be stopped and presently started afresh, but that the actual position of the polar axis should be affected.

The question arises whether we may not reject the Kentuckian's miracle, even though his announcement is prophetically worded. When we read in "King Solomon's Mines" of a solar eclipse which remained total for half an hour, to say nothing of its occurring a day after the moon had been full, which again occurred a day after the moon had been "new," we do not feel bound to accept the series of miraculous phenomena as the theologian accepts the standing still of the sun and moon for the accommodation of Jewish fighting folk. May we not in like manner suppose that the professor of Kentucky has made a trifling mistake? To put the matter plainly, may we not transfer our wonder from the details of his sensational announcement to the blatancy of his remarkable ignorance.

To speak seriously, the Kentuckian professor, in his holy zeal to be the first to proclaim the appearance of the Star of Bethlehem, has permitted himself the privilege of untruth. Doubtless he was, and is quite, certain that the star is due this current year; doubtless also he imagined that the star may begin to be visible to very keen eyesight some time before it becomes obvious to all. If the star really has a message for mankind, as no doubt the Kentuckian professor supposes, a man must be doing good service to the world who gets beforehand with the star and starts the message a

little sooner than the star could manage it. Picture the professor's state of mind with these ideas possessing him, and that perfect freedom of imagination which comes from sheer ignorance. How can he possibly carry out his idea better than by telling the world he has seen what Tycho Brahe actually saw? He turns, therefore, to the oft-repeated narrative of the discovery of the new star in 1572. Tycho Brahe saw the star in Cassiopeia; there, then, must the Kentuckian professor see it. Tycho Brahe saw the star near the zenith, so that Cassiopeia manifestly lodges overhead; there, then, our professor must see her. In unfortunate ignorance of the fact that a constellation which is near the zenith in November (Tycho Brahe first saw the star on November 11) is bound to be a long way from the zenith in May, our Kentuckian prophet saw Cassiopeia overhead, when she was really near the horizon; saw a new star where no new star existed, and—to speak in the language of the profane—developed a series of untruths until all outside was azure. Mr. Lockyer's achievement in telling little learners where the stars which pass overhead in London rise and set—stars which never come within a score of moon-breadths of the horizon—was nothing to the feat of this Kentuckian professor; for Mr. Lockyer only implied that he thought those stars might rise or set: our Kentuckian religionist tells us he actually saw Cassiopeia overhead, when she was in reality low down near the horizon.

But it may be asked whether, though this particular charlatan has not seen Tycho Brahe's star, that orb may not soon resume the abnormal splendour with which it shone in 1572 and 1573. This is possible (so many things are possible), but the evidence which led Goodricke to suppose that the star is an irregular variable, with a period averaging 312 years, is so slight that no astronomer would give much for the chance that Tycho Brahe's star will return to visibility this century, or perhaps for many centuries. In Tycho Brahe's time, a Bohemian astronomer (Cyprianus Leovitius was his highly respectable name), stated that in an old manuscript he had found records of the appearance of a new star between Cassiopeia and Cepheus in the year 945, and of another—or the same star—similarly situate in 1264. Even at the time much doubt was thrown over the account of these stars, several astronomers regarding it as probably relating to comets. But Tycho Brahe accepted the account as relating to stars, and considered that the object seen might possibly have been the same which blazed out so brightly in 1572.

That is absolutely all the evidence we have! Assuming the statement of Leovitius not wholly apocryphal, as some have not hesitated to assert that it was, we have statements about two bodies which may have been comets, but possibly were stars appearing in a tract of the heavens described as between Cassiopeia and Cepheus, a tract which must be assumed to be very broad indeed if it is to include the place of Tycho Brahe's star, in the years 945 and 1264—the intervals, if these were indeed apparitions of that orb, being 319 years and 308 years. This cannot be regarded as absolutely demonstrative evidence that Tycho Brahe's star is a variable blazing out, like a revolving signal light, at average intervals of 312 years, as Goodricke supposed, or 315 years, as others have suggested.

But even if, which is quite possible, the small star now under telescopic scrutiny for a quarter of a century because astronomers believe it to be Tycho Brahe's should blaze out suddenly with a lustre akin to that which it displayed in 1572, we may be well assured that the display, however interesting to astronomers and physicists, will not otherwise be a matter of the slightest moment for the inhabitants of this earth. To associate the idea of the systematic variation of some remote sun with remarkable events upon this little earth of ours, is to exhibit such an absence of all power of just reasoning as unfortunately characterises too many among the unscientific. It would not be possible to argue men out of such a belief, who, by the very fact that they have entertained it, have shown that sound reasoning (in such matters) is impossible for them. But as a mere matter of fact, the idea is as wild in its absurdity as would be the thought that the fortunes of a race of insects inhabiting a New Zealand tree must be affected by the systematic flashing out of the Eddystone Signal Light on the other side of the earth.

FREAKS OF THE WIND.

By HENRY J. SLACK, F.G.S., F.R.M.S.

NATURE often exhibits on a small scale actions which on a large one produce enormous effects. Thus when a heavy shower sweeps along the sand and gravel of a garden-path we have a miniature representation of what great floods do in transporting boulders and drift; and when tiny runnels cut a zigzag way through soft soil, being easily deflected by small obstacles, we have a copy of the way large rivers flow. With winds it is the same. The strong gales and cyclones which tear up trees, overthrow houses, and overwhelm ships, are sometimes represented by small aerial disturbances of a size that can be conveniently studied. The writer on one occasion, when driving home from a railway station, saw in front of him a shifting sand column about twenty feet high, small at the bottom and widening as it went up. It moved, rotating dancingly from one side of the road to the other, and fell to pieces as the vehicle passed swiftly by. If it had fallen on the travellers they would have been unpleasantly dusted, but innocent as its small power would have made it, as an illustration of the great dust-storms of the deserts it was better than any picture, because it was all alive with characteristic motion.

On a March day of the present year in South Devon a brisk strong wind from the north-east gave for many hours what may be called miniature rehearsals of the curious gyrations often performed with terrible consequences on a grand scale by wind and sea. The scene was on the top of the Down of Babbicombe, where in front of some terraces, and across a road of common width, is a long strip of grass constituting a favourite promenade, as it commands an extensive curving line of cliffs and shores of all colours, from crimson marl to pearly marble, yellow sand, and white chalk. In fact, it is the view of the coast from Oddicombe to Portland Bill. The wind came across some miles of sea. Part of its current may have rushed up the rocky cliffs, about 270 feet high; another part probably came straight to the grassy plain, and the two may have conflicted. Also some rebounding currents may have come back from the row of houses.

What happened was exhibited by sundry pieces of paper, small biscuit bags, and other reminders that light refreshments had been taken on the spot. The same pieces of paper were made to perform all day in a space of some thirty or forty yards long, and half that width. Moreover, they never mounted high in the air. Any one would have thought that a strong blast from north-east would at once have carried them off south west, but it did nothing of the kind. A glance at the pennon on an adjacent flagstaff showed the wind generally true to its north-east line of approach, but it went all ways on the grass. Frequently pieces of paper, of about the same size and shape, and only a few

feet apart, were carried simultaneously in opposite directions. Sometimes two or more pieces went round in circles; at others, two of the biggest hopped rapidly about each other like lively birds at play, but none rose many feet from the ground. It frequently happened that the velocities of their movements varied considerably beyond what could have resulted from impacts of air-currents of the same force upon objects differing slightly in size, weight, and shape. It was plain that the wind moved in narrow streams, varying in velocity, so that objects a few feet apart received blows of very different force.

I cannot remember distinctly enough for exact reference where I saw an account of experiments which illustrated this behaviour of a wind-storm. So far as I can recollect, screens of small dimensions were placed in line, and so arranged as to record the force of the wind impact. The result showed that the air-currents struck the screens much as if a quantity of balls, big and little, had been hurled at them.

Storms of very limited breadth are known to be of common occurrence, and while disastrous damage has been done to trees and buildings within a few yards of each other, adjacent objects have had no strain put upon them. What the wind at Babbicombe did was to imitate over and over again this sort of action on a minute scale.

Probably in all violent cyclones it is a mere chance whether any of the usual anemometers happen to receive and indicate the extreme force with which some part of a much larger object would be struck. Some years ago a storm of small breadth in its most violent part overthrew my observatory, made of wood strengthened with T iron and covered with stout canvas, while no damage was done to trees a few yards off. An anemometer near the trees would not have indicated anything like the force which lifted up and carried forward the observatory.

It would be well worth while to erect in some situation exposed to storm-winds a screen representing a section of a house or bridge, and arranged like the one mentioned, in registering compartments. We might thus obtain a better idea of the forms and the forces of wind currents, and of the resisting power required for safety.

FORCE AND ENERGY.

ALTHOUGH so frequently used by scientific and unscientific writers, the words "force" and "energy" are constantly confounded. I find many misinterpreting, on this account, passages in which the words are used. It may, therefore, be worth while to indicate the sense in which these words are to be understood in accordance with the meanings now definitely assigned to them in scientific writing. They should, in reality, be no more confounded as thus used than gravity and heat—gravity is, indeed, a form of force, and heat a form of energy. Force is any cause by which motion is produced or modified. A force in thus moving or modifying motion does work. The work stored up, as it were, in a body, through the action of a force upon it, is called *energy*. If I throw a stone into the air I exert force upon it; the velocity I communicate to it represents the work done upon it, and this velocity possessed by the body, at the moment when it leaves my hand, represents the energy of the mass which, before I threw it, had been inert. The energy possessed by the body may seem to vary, and actually does vary, but not quite so much as it appears to. It varies because owing to the resistance of the air the energy of the body is continually being diminished; but the energy thus lost by the body is transmitted to the air. But the further loss of velocity on account of the earth's attraction retarding the velocity of the body which had originally been thrown upwards, is not a loss of energy, for the change of place involves a corresponding increase of what may be called energy of position, and this velocity will be presently restored as the attraction of the earth draws the stone down again. In imparting velocity to the stone, again, I was not increasing the stock of energy in the universe; for by whatever upward action I drove the stone from the earth, I correspondingly (regard being had to the relative mass) urged my own body towards the earth, depriving my body of just as much energy of position as corresponded with the energy I imparted to the stone. Energy is, in fact, akin in one respect to mass—it may be taken from or imparted to a body, or from a system, or imparted thereto; but its totality can never be altered, any more than the total amount of matter in the universe can be altered.

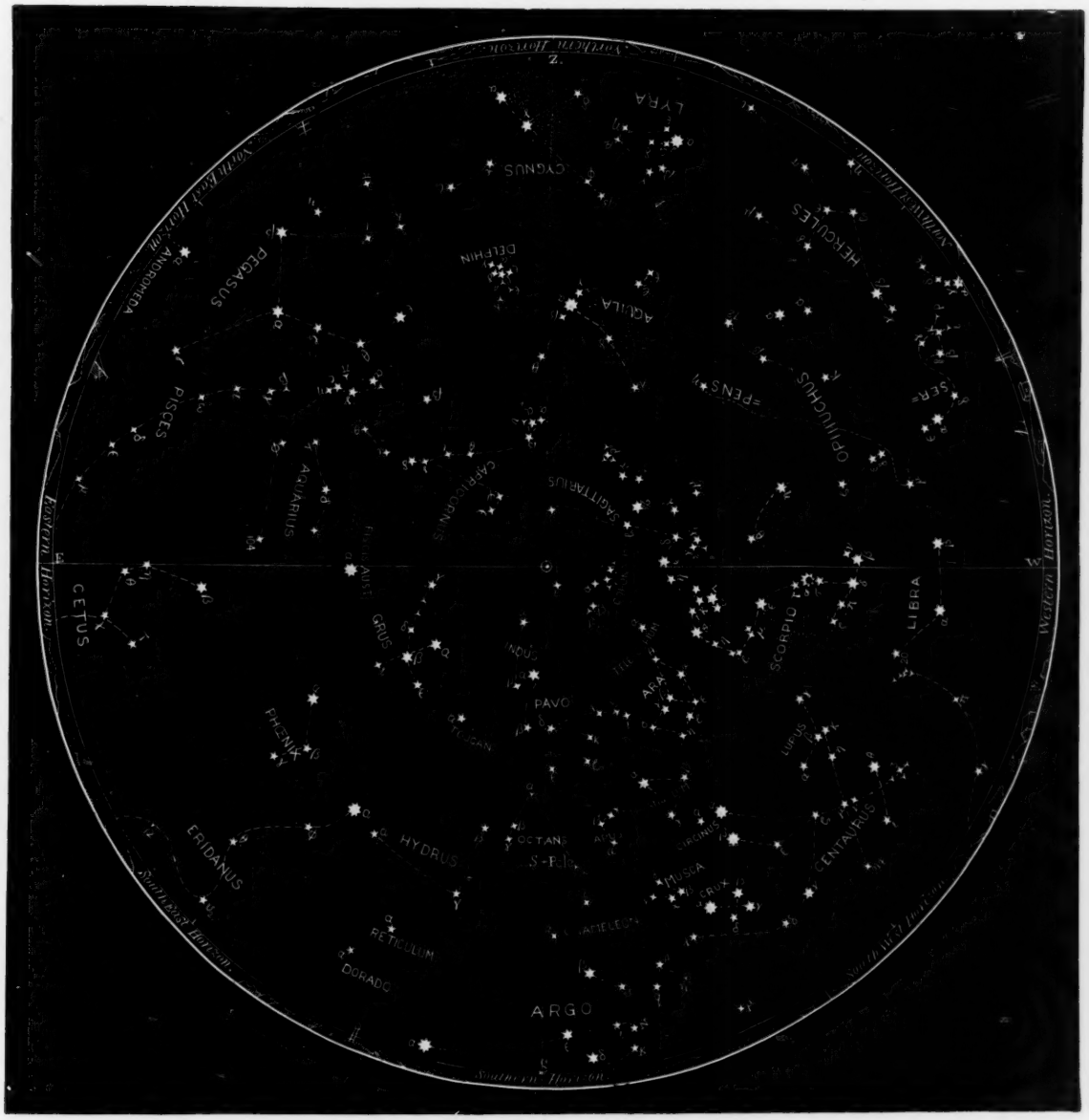
It has been stated that with regard to these words "force" and "energy" science has no settled usage, but there is no foundation whatever for this statement. The use of the words has long been definitely fixed in scientific treatises. Those unfamiliar with mathematical and physical science are very apt to misuse both words, as also to substitute for one or other words which have no such definite scientific use—as power, might, influence, and so forth. But *abusus non tollit usum*, the abuse of anything, as Guy Mannering puts it, doth not abrogate the lawful use thereof.

Probably the confusion which has arisen among outsiders respecting these two words "force" and "energy" has been caused in great part by the varying inter-relations of force and energy as cause and effect at one time, effect and cause at another, though, indeed, one ought rather to say that they appear at one time as following and antecedent, at another as antecedent and following, respectively. Yet, in reality, we must regard energy as the true antecedent, seeing that in its fullest sense energy is (as defined by Rankine) the capacity to effect changes. When force is exerted, energy, as we have said above, is stored up somewhere in precise proportion to the exerted force: but that force was itself the product of pre-existing energy. Nor do we ever recognise force which has not in some way been generated by the exertion of some form of energy, though, as in the case of the attraction of gravity, we may be unable to say what that form of energy was, or how it may have been exerted.

DOUBTING FAITH.—Of old it was held to be a dangerous thing to inquire too closely into the nature and origin of the earth on which we live. Men might inquire, though only with caution, into the natural processes taking place around them; into the growth of plants and animals; or into the origin of winds, rivers, and other such phenomena. It was not held, for example, to be an obvious sign of Atheistic tendencies to discuss the development of a tree from the seed, or to trace the progress of a river from its source. But a warning voice was heard so soon as inquiries were pushed a little farther. If observant men, for instance, were led to examine the formation of a river-valley, and thence to inquire under what forces that valley had assumed its present form, they were told that they were pushing their queries beyond the verge of what man was suffered to understand. Worthy people in those days seem to have laboured under a continual fear of discovering too much, of coming unexpectedly upon the operation of a first cause. They seem to have felt that as Moses took off his shoes when he approached the bush, so it behoved them to proceed cautiously and as with unshod feet, lest unwarily they should light on evidence of the direct action of the Creator's fashioning hand.

THE SOUTHERN SKIES.

MAP IX.—FOR JUNE, JULY, AND AUGUST.



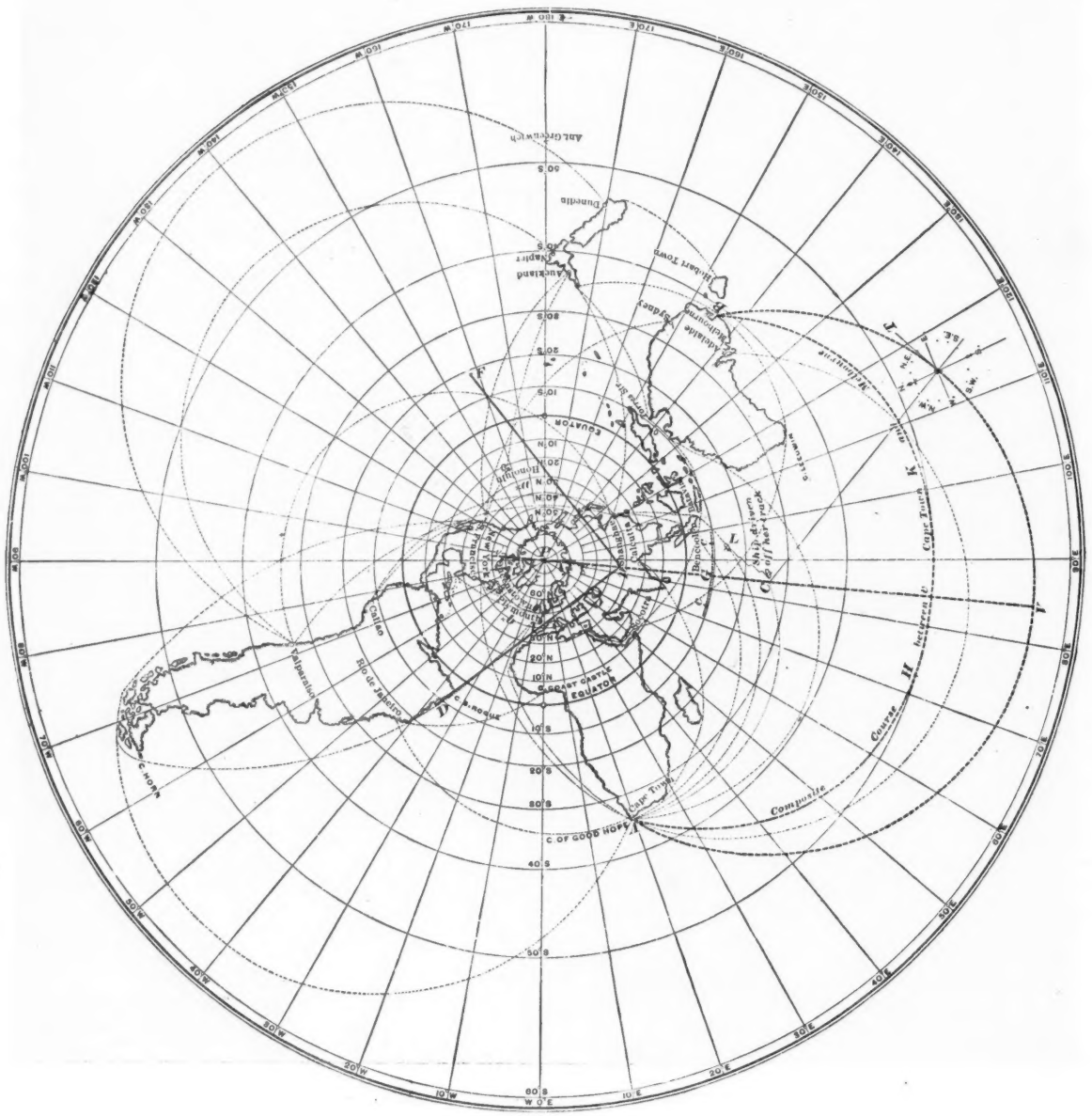
THE NIGHT SKIES IN THE SOUTHERN HEMISPHERE (LAT. 46° TO 24° S.)
AND THE
SOUTHERN SKIES IN ENGLAND (UPPER HALF OF MAP ONLY) AT THE FOLLOWING TIMES:

At 1 o'clock, morning, July 7.	At 11 o'clock, night, Aug. 7.	At 9 o'clock, night, Sept. 6.
„ 12.30 „ „ July 14.	„ 10.30 „ „ Aug. 14.	„ 8.30 „ „ Sept. 14.
„ Midnight, July 26.	„ 10 „ „ Aug. 22.	„ 8 „ „ Sept. 21.
„ 11.30 o'clock, night, Aug. 3.	„ 9.30 „ „ Aug. 29.	„ 7.30 „ „ Sept. 29.

STAR MAGNITUDES.

First . . . . ✸ Second . . . . ✱ Third . . . . ★ Fourth . . . . + Fifth . . . . ▲

XUM


Auckland
Hobart Town
Sydney
Adelaide
Melbourne
Cape Town
Buenos Ayres
Rio de Janeiro
Falkland Is.
C. Horn
C. of Good Hope
Great Circle
Equator
Course
Composite
Course
Great Circle
Course
Great Circle
Course
Great Circle

QUARTER OF THE SOUTHERN CHART on the same Projection (and on the same scale), showing fourteen illustrative great circle tracks in the North Atlantic, obtained by the same method. [L and K mark the points where the steamships from Queenstown and from Bordeaux respectively cross the Newfoundland Bank.]

HOW TO USE THE CHART

I. To find the *great circle course* between two places, A and B.

Describe a circle through A, B, and *a* the antipode of A (or through A, B, and *b* the antipode of B); it will pass through *b* (or *a*) and be *the great circle required.*

II. To find the *vertex* or highest latitude reached.

A straight line through the pole and the centre of AB*ba* cuts this great circle in *the highest latitude point required.*

III. To find the bearing at any point of the course.

The course cuts the Meridians at angles *showing the true bearing.*

IV. To find the *composite course* between A and B, touching lat. *l.*

With radius equal to half the distance, across pole on chart, between north-latitude *l* and south-latitude *l*, describe arcs through A and B touching lat. *l*; these arcs, and part of lat. parallel (*l*) between them, make up *the composite course required.*

V. To find the great-circle distance between A and B.

Find *p* the pole of the great circle course (90° in lat. from vertex), and centres D and F of great circles through A*p* and B*p*; then *the great circle course AB contains as many degrees as there are in the supplement of DpF, each degree containing* 60 *geographical miles.*

NOTE.—If a ship is driven from her course, as from L to C, the great circle course from C (as CA) is found by I.

EXAMPLE.

To find I., the *great circle course*; II., *its vertex*; III., the *bearing (at any point)*; IV., the *composite course* (*lat.* 50); and V., the *distance*; from Cape Town A (antipode *a*), *to Melbourne B (antipode b).*

I. Find C, the centre of circle through A, B, *a*, *b*, and describe, round C, *the required great circle course* AVB.

II. Through P, C, draw PC*v*V′, giving the *vertex*, V.

III. Draw compass-card N-E-W-S about O,NS meridional, then OT, tangent to course, shows the *bearing at O.*

Note.—Angle EOT = angle NOC.

IV. Bisect *vv′* (lat. 50 N & S) in G; *about* P describe arc *c*G*c′*; with radius G*v* and centre on *cc′*, desc. arcs AH, BK; AHVKB is the *required composite course.*

V. Take *p* on PV, 90° from V; find D, the centre of a circle through A, *p*, and *a*; and F, the centre of a circle through B, *p*, and *b*; *AVB contains as many degrees as the supplement of the* angle *DpF.* By measurement with a protractor, AVB contains $92\frac{3}{4}°$, corresponding to 5,565 geog. miles. True distance $5{,}566\frac{5}{6}$ geog. miles (587 geog. miles less than Mercator's course).

CHARTS FOR GREAT CIRCLE SAILING.*

SHOWING AT ONCE THE GREAT CIRCLE TRACK, THE COMPASS BEARING AT EACH POINT, AND THE DISTANCE; ALSO THE "COMPOSITE COURSE" TOUCHING ANY GIVEN LATITUDE.

BY RICHARD A. PROCTOR.

THE shortest distance between any two points on a globe is the lesser arc of the great circle passing through them (a great circle on a sphere being one whose plane passes through the sphere's centre). But the seaman, in passing from port to port on the earth, generally follows what is called a rhumb line—such a track that he has the same compass course (apart from magnetic variation) throughout his journey. Mercator's projection, on which the charts of the world in our books of geography are drawn (I mean those charts which show the whole world), was invented to help the sailor in marking his true rhumb course from port to port, this course being shown in Mercator's charts as a straight line. In long journeys, however, especially such as are made in the southern hemisphere, the rhumb course is far longer than the great circle course. For instance, from Cape Town to Melbourne the course on a rhumb line is 587 miles longer than the course on the arc of a great circle. Even in such a journey as from Queenstown to New York (where, however, the great circle track is broken by the Newfoundland shores, and two arcs have to be combined) there is a considerable saving of distance in following the great circle route. Moreover, for sailing vessels tacking against adverse winds the saving is far greater. In tacking along a rhumb course, sailing as close to the wind as she can, a sailing vessel is often actually increasing her distance from her haven. In passing from the English Channel to New York, on a rhumb course, against adverse winds, a sailing vessel tacks over 7,360 miles; but taking the great circle course, the distance traversed in all her tacks would be only 6,490 miles, a saving of 870 miles, or five or six days' sailing for a craft of medium speed! On some of the long South Sea journeys, where the difference in miles between the rhumb course and the great circle course may be seven or eight hundred miles, the actual difference of distance traversed in tacking against adverse winds would amount to two or three thousand miles!

My present object is to show how charts may be made which will be as convenient for great circle sailing as Mercator's charts are for sailing on a rhumb line.

Two difficulties have checked the extension of the system of great circle sailing. In the first place, a process of calculation has to be gone through to determine even the proper *first* course for great circle sailing, that is, the bearing at the port of departure; and a fresh calculation has to be made for a succession of points—usually taken five degrees in longitude apart—along the great circle course. Secondly, it often happens that the true great circle course would carry a ship into inconveniently high latitudes.

To meet the first difficulty, various methods have been from time to time suggested for obtaining graphically the great circle course. Sir George Airy invented a most ingenious but unfortunately a complicated construction for drawing the great circle course on a Mercator's chart. Mr. Hugh Godfray, the author of the Cambridge text-book on the Lunar Theory, suggested the use of the gnomonic projection, where the great circle course is represented by a straight line, just as the rhumb course is represented on a

* This article is mainly a reprint from the *Scientific American*, from whose pages the illustrations have been reduced.

Mercator's chart; and various approximative constructions have been suggested from time to time.

To meet the second difficulty, Mr. Towson, of Liverpool, suggested composite sailing, by which a great circle course is taken from the port of departure to touch the highest latitude deemed safe, and another great circle course is taken which, touching that highest latitude, passes through the port of arrival; the journey pursued is along the former great circle course to the limiting latitude-parallel, then along this parallel till the second great circle course is reached, and thence along the latter great circle course to the desired haven. Mr. Towson published also a valuable series of tables for facilitating the calculation of the composite course from port to port. The Rev. George Fisher, chaplain of the Greenwich naval schools, devised a graphic method for approximating to the composite course on a Mercator's chart.

These methods of calculation and of construction have not come into general use. It has been found impossible to introduce the general use of great circle sailing as hampered by these requirements, especially in the case of sailing vessels, where fresh calculations or constructions, by no means simple, would have to be made whenever a ship had been driven out of her course by stress of weather.

A chart on which the great circle course between any two points can be at once laid down would obviate these objections. And at first sight it seems as though Mr. Godfray's proposition met this want; for, as I have said, the great circle course on a gnomonic chart is a straight line. But a gnomonic chart cannot show even a full hemisphere. The point of projection is at the centre of the sphere, as shown at O, Fig. 1, the projection being made on a tangent

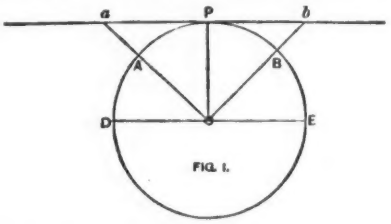


FIG. 1.

plane as aPb. Supposing one of the poles to be at P, the centre of the projection, the points A and B would be projected at a and b respectively. The points D and E could not be projected on the plane aPb at all. The scale of the chart increases rapidly from P outward, becoming infinite for points 90 degrees from P. The projection then, though it might serve for ports in one hemisphere, is evidently not available generally. Moreover, the gnomonic projection would not indicate the bearing (anywhere on the course), the vertex, or the distance.

Instead of this practically useless method of constructing charts for great circle sailing, I propose the use of the stereographic projection, whereby (1) the construction for marking in the great circle course between any two points is made exceedingly simple; (2) the whole course is obtained at once; (3) the "bearing" at each point of the course is shown as plainly as the bearing of the rhumb course on a Mercator's chart; (4) the "composite course" where wanted can be obtained by a simple construction; and (5) the distance from point to point can be easily determined.

I need not here discuss the principles of the stereographic projection at any length. I will simply note those points which make the projection convenient for the proposed purpose.

In the stereographic projection of the sphere, the point of projection O, Fig. 2, is on the surface of the sphere, at the extremity of a diameter PCO, through P, the centre of projection. Thus if d P f represent the tangent plane through P, the points A and B on the sphere would be projected on d P f at a and b, where OA and OB produced meet d P f. If D and E are 90 degrees from P (as in

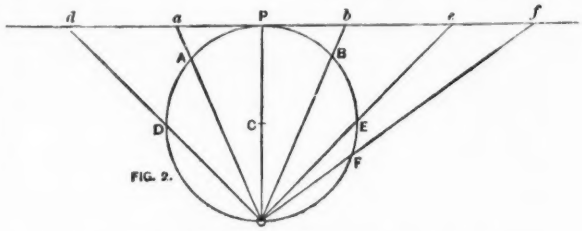


FIG. 2.

Fig. 1), their projections fall on d P f at d and e. A point as F, still nearer to O, will be projected on the tangent plane as at f.

If P be either pole, the projection of the sphere on the plane d P f is a very simple matter; for all the meridians are projected into straight lines through P, and all the latitude-parallels into circles around P as centre. The radii of these circles can be obtained by construction, as shown in Fig. 2. But in practice it is far better to use their known lengths as indicated in trigonometrical tables. Thus if PB is an arc of 60°, we know that the angle POB contains 30°; so that P b is equal to PO tan. 30°. Thus, for the parallels corresponding to latitudes 85°, 80°, 75°, and so on, we take from the trigonometrical tables the natural tangents of $2\frac{1}{2}$°, 5°, $7\frac{1}{2}$°, and so on; and these numbers, with any convenient unit of length, give us the radii of the circles we are to describe round P. For instance, if we wish the equator to have a radius P e (equal to PO) five inches in length, we draw a line five inches long, divide it into ten equal parts, and one of these again into ten parts (or preferably make a plotting scale for the smaller divisions); then regarding one of the tenths of the line, *i.e.*, one-half an inch, as our unit, we take, for our successive radii, lines having the following lengths:

For latitude	85°	0·437 which is the tang. of		$2\frac{1}{2}$°
„	80°	0·875	„	5°
„	75°	1·317	„	$7\frac{1}{2}$°

and so on. Then a series of radial lines drawn to divisions 5° apart round any one of these circles give the meridians, and complete our projection. The chart should have outlines of continents, islands, &c., marked in, for convenience, though in reality this is not essential, because the longitudes and latitudes of ports, &c., are alone really needed for determining the great circle course, and the course obtained by the simple constructions I shall indicate could always be plotted in on the Mercator's chart, to whose use seamen are more accustomed than to that of any other kind of chart.

The properties of the stereographic projection, which enable us at once to project a great circle course, and to determine bearings, distances, &c., on a stereographic chart, are the following:

(*a*) Every circle on the sphere, great or small, is projected into a circle.

(*b*) All angles, bearings, &c., on the sphere are correctly presented in the projection (a property found also in Mercator's projection).

With these properties I combine the following properties of great circles on the sphere:

(1) Since every diameter of a great circle is a diameter of the sphere, each point on a great circle is antipodal to another point on the same circle; or, in other words, if a

great circle passes through any point, it passes also through the antipodes of that point.

(2) If a great circle touches a small circle on the sphere, it touches also the small circle antipodal to the former; for instance, if a great circle touches latitude-parallel 30° north, it touches also latitude-parallel 30° south. This needs no demonstration, being really a corollary of 1; for the point in which the great circle touches one small circle has for its antipode a corresponding point on the antipodal small circle, and also, by 1, on the great circle: and there can be no other point in which the great circle meets the antipodal small circle; for if there were, then, by 1, there would be corresponding points of contact or of intersection with the original small circle, which by our hypothesis is not the case.

To use the charts, however, the seaman need not concern himself either with the method of constructing them or with the principles on which their use in great circle sailing depends. All he need care for is rightly to apply the constructions which result from these principles.

I propose to indicate only what are the processes necessary for the five following problems:

I. To find the great circle course between any two points, as A and B, Fig. 3.

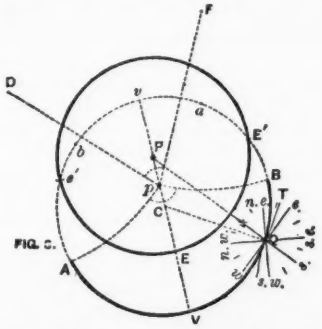
II. To find the vertex, V, or highest latitude reached, on that course.

III. To find the bearing at any point, as Q, on the course.

IV. To find the composite course, from port to port, touching any given limiting latitude.

V. To find the distance, A V B, to be traversed.

The constructions for these five problems are all included in the following simple statements (P, Fig. 3, is the pole,



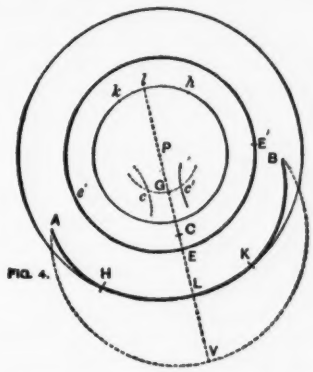
e'EE′ the equator), the italicised parts indicating the actual constructions, the rest giving the reasons and demonstrations:

I. *Find on the chart a and b the antipodes of A and B* (which of course is easy, as we have only to take off 180° on the meridians A P *a*, B P *b*, not shown in the figure to avoid crowding); then *a circle through any three of the points A, a, B, b, is a great circle of the sphere by* 1, *and must pass through the fourth point. Describe such a circle A B a b; A V B is the great circle course required.* [It will be best to take the three points A, B, and *b*, A being the point of departure. Pencil the bisecting-perpendiculars to *b* B, AB (not shown in Fig. 3), intersecting in C, around which point as a centre describe in pencil the circle AB*b*. Note whether it passes through *a*, for this serves as a test of the accuracy of the result. You may also note whether the points e' and E′, in which it cuts the equator, are, as they should be, on the extremities of a diameter through P; for two great circles on a sphere necessarily intersect on a diameter of the sphere, and therefore, as P is the projection of the pole of the equator, two such points of intersection must lie on a straight line through P.]

II. *A straight line vPCV, through P and C, cuts the great circle course in V, the vertex required,* V being the highest latitude on one side of the equator, *v* that on the other. [V does not necessarily fall on the actual great circle course between two points. For instance, QB is the great circle course from Q to B, but V lies outside QB.]

III. *Draw QT, tangent to the course, at Q. Then the angle PQT gives the bearing of the course at Q from the due northerly direction QP.* By observing that QT is at right angles to CQ, we can get the bearing without actually drawing QT. Thus in the case illustrated in the figure, the direction QT is north of due east by an angle equal to CQP, easily measured with a protractor.

IV. Suppose AVB, Fig. 4, the great circle course, to have its vertex V in inconveniently or dangerously high latitudes. *Let* L *on* PV *be the highest latitude which the ship must reach. Take l, antipodal to* L; then if we *bisect l* L, *in* G, and describe round P as centre the circle *c* G *c*′, it is obvious that any circle having its centre on *c* G *c*′, and radius equal to GL or G *l*, will touch both the latitude-parallels



HLK and *klh*; and, by 2, will be a great circle. Therefore, *around* A *and* B, *as centres, with radius* GL, *describe circles cutting c* G *c*′, *in c and c*′, *and with c and c*′ *as centres and the same radius describe circular arcs,* AH, BK, *touching the latitude-parallel* HLK *in* H *and* K. *Then* AHKB *is the composite course required.* The distances along AH and BK can be determined by the method shown in the succeeding section, and the distance along the latitude-parallel HLK is of course easily determined, being an arc of a known number of degrees in a known latitude.

V. [We have to determine how many degrees there are in the arc AVB, not as it appears in the chart, but as it really is on the sphere.] *Take p,* 90 *degrees of latitude from* V *and v, Fig.* 3, *along v*PV. (This of course is done at once on the chart, which shows the degrees of latitude from the pole P.) Then *p* is the pole of AVB. *Find* D, *the centre of the great circle* A *p, and* F, *the centre of the great circle* B *p.* (This we do by I.; but most of the work is already done. We have the bisecting-perpendicular to B *b*; the bisecting-perpendicular to A *a* passes through C; then the bisecting-perpendiculars to A *p*, *p* B, give us—by their intersections with those to A *a* and B *b*—D and F at once.) What we want is to determine *the angle* A *p* B, between the arcs A *p*, *p* B; and it is obvious that this *is the supplement of the angle* D *p* F, which is easily measured with a protractor. *The number of degrees, multiplied by* 60, *gives the number of geographical miles, or knots, in the distance* AVB.

An example of these methods is given in the accompanying

stereographic chart, where the same letters are used as in Figs. 3 and 4.

[By a singular coincidence the distance actually determined by my construction for the great circle course from Cape Town to Melbourne was 5,567 miles, the nearest result in whole numbers to the distance determined by calculation, viz., 5,566⅝ miles. But having obtained the angle 92° 47′, I did not consider anything closer than 92¾° could fairly be used, giving the distance 5,565 miles (very close for a determination of this sort).

The distance between Cape Town and Melbourne on a rhumb course is 6,154 miles, *or* 587 *miles longer than the great circle course!*]

The illustrative chart shows, on a small scale, my northern chart on the plan described. It has been carefully drawn, with meridians and parallels ten degrees apart correctly placed. The distortion is great outside the equator, but not greater than for high latitudes in Mercator's chart; nor does distortion at all affect the utility of such charts. (The stereographic and Mercator's projections are alike in showing small regions with little distortion.) More than thirty great circle-paths are shown, but of course the charts which I have had prepared for sailors' use (under Mr. E. Stanford's supervision) are without paths of this sort, the object of the charts being to enable the seaman to lay down without trouble the track he has to pursue in order to traverse the shortest distance from any port whatever, *or from any point he may have reached on his journey*, to any haven.

The north pole being in this chart the centre of projection, the northern regions are on a relatively small scale. Thus, the few great circle tracks shown on the Atlantic are not so well presented as the tracks in the southern seas, or from the northern to the southern hemisphere. The meridians and parallels of the chart serve, however, equally well for a map of which the south pole would be the centre. I have had a second chart prepared on this plan (the distortion affects Asia and North America very curiously), and the smaller map (a quarter of the chart) shows the North Atlantic as presented in this chart, the projection and scale being the same as for the larger chart. Although my large northern chart would suffice without any supplementary southern chart, yet the southern scale adds greatly to the completeness of the work. Moreover, by applying rule I. for pencilling the course on both charts, and comparing the results (which ought to agree, of course, exactly) the seaman will escape all possibility of any error, however slight, and will find his confidence in the accuracy of the method strengthened, should he, by chance, be unable to understand the mathematical reasoning on which the method is based.

THE VOYAGES OF COLUMBUS CHARTED.

BY RICHARD A. PROCTOR.

TO illustrate at once the convenience of the one-scale maps now appearing monthly in KNOWLEDGE, and the advantage of great-circle routes across the ocean, I give a map (No. 1) on the same projection in which the routes followed by Columbus across the Atlantic have been laid down for comparison with the corresponding great circle routes. The map also shows the shortest routes between several places in Europe and America, together with the rhumb course between Queenstown and New York (the course indicated as the shortest in Mercator's charts, but really not so).

On Friday, August 3, 1492, Columbus, in command of a squadron consisting of the *Santa Maria* (a decked vessel) and his flag ship, the *Pinta*, and the *Nina*, set sail from Huelva on his first journey across the Atlantic in search of a westerly route to "far Cathay." In the same year a globe was made at Nuremberg, from which the accompanying map (No. II.) has been formed. By comparing it with map I., which is in fact a chart of the North Atlantic, some notion can be formed of the ideas which were entertained by the most experienced geographers in the days of Columbus about the unknown Western Seas. About as far west from Europe as the shores of the United States really lie, we find Cathay, a small island, and Cipangu, a larger one, doing duty, but most inefficiently, and far from their proper latitudes, for China and Japan. What island it may be, to which Sieur Brandan came in the year 565, it would be difficult to determine; but neither Behem nor anyone else in those days had clear notions about the distances traversed by voyagers who reported shores they had sighted. However, this island was reported to have been seen by travellers sailing south-west from the Cape Verde Islands, and is therefore probably altogether mythical.

On August 9 Columbus sighted the Canaries. It was not, however, till September 6 that the squadron sailed from Gomera, nor till Sunday, September 9, that the heights of Ferro, the most westward of the Canaries, faded from view. On September 13, about six hundred miles due west of Ferro, Columbus noticed the variation of the needle, five or six degrees eastwards of true north,* a deviation which increased as he proceeded.

On September 14 the voyagers saw a heron and a tropical bird which they regarded as harbingers of land; but, on the following night, they were alarmed by a meteor, which they held to be ominous. Columbus explained the meteor after the simple manner of his time, when science understood everything; Irving, however, condescendingly remarks that these objects are common in warm climates, a statement about as instructive as though he had said that planets are peculiar to the subtropical skies. On September 20 the wind, hitherto from the east, veered to the south-west, and they altered their course (when in about 40° west longitude) slightly northwards. Several birds, regarded as denizens of the land, visited the ships, and next day the squadron reached the Sargasso, or great seaweed bed, which they endeavoured to avoid. On the 25th the wind again became favourable, and for several days thereafter they sailed steadily south-westwards, taking an almost due westerly course on September 30. On October 6 Martin Pinzon, commander of the *Pinta*, proposed that they should stand more to the southward, and in the evening of October 7 Columbus altered the course very slightly southwards, being then in about west longitude 65° from Greenwich. For three days they stood in this direction, the signs of land becoming more and more frequent. The crews, however, regarded these signs as meant to lure them to destruction; and Columbus, who had hitherto kept his men in good spirits by gentle words and promises of reward, was compelled for the first time to adopt a sterner demeanour. This was on Wednesday, October 10. On the 11th the signs of land became unmistakable. It was at about ten, on the night of Thursday, October 11, that Columbus saw a light glimmering at a great distance, which he showed to Pedro Guiterrez, "gentleman of the king's bedchamber," whatever dignity

* Washington Irving says the needle pointed to the north-west, but that is wrong: what Columbus found was that the true north lay to the west of the point indicated as north by the magnetic needle; probably Irving was misled by the circumstance that now the needle points west of north at the place where Columbus first discovered its deviation. The magnetic pole was considerably east of the latitude of Greenwich in the year 1492.

that may imply. At two on the morning of Friday, October 12, a gun from the *Pinta* announced that land had actually been sighted; and in the morning a level island was seen, several leagues in extent, and covered with trees like one great orchard. The inhabitants could be seen issuing from all parts of the woods, and running down to the shore.

This island, still called San Salvador, as named by Columbus, is also called Cat Island, after the less euphonious and dignified name which the English masters of the Bahamas have thought appropriate. The light seen by Columbus was probably on Watling Island, which lies a few miles to the east.

After voyaging about among the Bahamas, and discovering St. Domingo, Columbus sailed for home on January 16, 1493. But it was not in the *Santa Maria* that he made the return journey. She had been lost through the negligence of a boy left in charge of her, and Columbus transferred his flag to the *Nina*, in which, after narrowly escaping destruction during a great storm, he finally reached the harbour of Palos, on March 15, 1493, having been absent seven and a half months.

MAP I.

The map shows how much his journey to San Salvador might have been shortened had he known how the land lay. For in maps on this projection the great-circle or shortest courses from place to place are represented very nearly by straight lines, as the meridians show (which are all great circles); and it is only necessary to compare the actual

length of the track followed by Columbus from Huelva to San Salvador with the shortest route shown—running across the easternmost of the Azores—to see how far Columbus went out of his way.

But apart from any trustworthy information as to the position of Cathay, Columbus might, with a little knowledge of the properties of the sphere, have recognised that his shortest course to the East Indies, as pictured in the maps of his day, required him to run as far northwards as the course marked in Map I. from Cape St. Vincent to St. Augustine, Florida. By following such a course he would have been brought, after rounding the Azores on the north, to more southerly latitudes, and finally into the latitudes of the

MAP II.

Canaries, somewhere in west longitude 85°—before reaching which, however, he would have fetched the shore of Florida.

The other outward journeys made by Columbus were not less circuitous. The student of Washington Irving's Life of Columbus will find it interesting to follow these journeys on such a projection as Map I., or, even better to mark it in, as shown from day to day on Mercator's charts, on the map of the Atlantic at page 106, where the currents of that ocean are also shown. In the second journey Columbus, after first discovering the North Caribbee Islands, passed within the Caribbean Sea, and discovered Jamaica and Cuba. On the third journey he would doubtless have discovered the mainland of South America, had not provisions run short, compelling him to take a more northerly course for the region where he knew land lay. He thus reached Trinidad, so named by him because of a great triple mountain on the island. The fourth journey took him to the South Caribbee Islands, and, again passing into the Caribbean Sea, he reached the mainland near Cape Honduras; but Ojeda had reached the mainland of South America (probably Surinam) in 1499, nearly three years earlier.

It will interest the reader to compare together the various tracks across the Atlantic shown in Map I., all of which, except the rhumb course from Queenstown to New York, are the shortest routes between the places they connect.

The map has been drawn specially, however, to illustrate a little work on America which I am now preparing. It will be observed that only the thirteen States which proclaimed their independence more than a century since, and being chiefly of British blood most manfully maintained their independence and established it, are shown on the westward side of the map. The area which our British colonies then occupied can be recognised at a glance, and compared with the area of Great Britain and Ireland on a map such as this, whereas no ordinary atlas conveys clear ideas on this point. We see also, what no map hitherto drawn has I think properly shown, not only the relative areas of the home country and its colonies, but, properly represented to scale, the distances separating the Britons at home from their kinsmen across the Atlantic. But for this convenient remoteness, our gallant fellow-countrymen on the other side of the Atlantic might haply have had to wait for a generation or two longer before they were able successfully to get rid of the blundering sway of the crazed German George III., supported by a large but silly section of the folk at home.

ORIGIN OF LANGUAGES.

AMONG the curiosities of the science of our day a chief place must be assigned to the evidence which has been obtained from the study of language in regard to the past history of various races. If we consider the crude attempt made by a Semitic race to interpret the variety of human methods of speech, and contrast that quaint story of the dispersion of man with the conclusions resulting from the careful study of the evidence given by languages themselves, we find something truly marvellous in the progress men have been able to make in dealing with the mystery of language. The story of Babel is like giving up the riddle; the results already obtained by philologists seem by comparison like a complete answer, though in reality they bring before us difficulties greater far than those they remove. In this, however, the study of language resembles all other study. All knowledge really worth anything increases our consciousness of ignorance.

It is from language alone we know that the Indians and the Persians are nearer of kin to us than the Jews or Syrians, for the beginnings of all four races are lost in a remote antiquity. But the connection between the Indian language (even in its ancient form as Sanskrit) and modern languages of the Teutonic family is not obvious until the rules for making comparison are recognised. For instance, one would hardly suppose that the relationship between the Sanskrit *vrkas* and our English "wolf" could be shown to be demonstrably real, or in fact more than the merest guess. Yet this relationship is only one of a set, and when we take the same word in various Indo-European languages, we find that the law of the connection shows itself even on the strength of this one word alone, though, of course, for complete demonstration other words associated in the same way have to be considered. Thus we have:—

Sanskrit	*vrkas*	Old Bulgarian	*vlukir.*
Greek	*lukos*	Gothic	*vulfs.*
Latin	*lupus*	Old High German	*wulf.*
Lithuanian	*vilkas*		

Even the connection between Greek and English is by no means obvious till the law of kindred is pointed out. The connection between *pater* and *father* is indeed easily recognised; but one does not at once see how *děka* and *ten* are connected, yet nothing is more certain than that these two words not only mean the same number, but mean it in the same way. To trace the connection we have to go back to the Gothic *taihun* or two hands, from which our "ten" is unquestionably derived. But even *taihun* differs slightly from *děka*. We notice further, however, the Latin *decem*, the relationship of which to the Greek *děka* is certain (though affected by our mode of mispronouncing the Latin "c"). Now, the Teutonic "h" was the equivalent of the Greek and Latin k or c, as *horn* for *cornu* shows, or the Gothic *hund* for *canis*, or *tiuha* for *duco* (or, conversely, *garden* for *hortus*=Greek *chortos*). So that we recognise in *decem* and *děka* words indicating two hands, as, indeed, the "ten" in any language is bound to do. The word "ten" is fully as much altered from the original *two-han* or *taihun*, as *decem* from *duo* and *ken*, or *hen*, which last root we recognise only in its compounds, *prehendo*, *comprehendo*, &c.—the old verb *hendo*, to grasp with the hand, having early disappeared from the Latin. Of course, in *canis* we have the idea of grasping just as we have in our English *hound*, the connection of which word with *hand* is undoubted. In passing we may note the connection between *prize* and *hand*, which seems at first sight incredible, but properly looked at is obvious, being simply shown by the series, *prize*, *prise* (Fr.), *prendre*, *prehendo*, *henden*, hand.

One might cite, again, the connection between the Greek *pente* and the English *five*. At first sight there seems no connection at all. But between the old Greek *pempe* (as in Homer) and the Gothic *fimf* the connection is clear enough; and no one can doubt that our *five* is derived from the early Teutonic *fimf*. In the Latin we have *quinque*, *q* replacing *p*, instead of *f*, as in the Gothic. But *qu* was simply a strongly gutturalised *w* sound, as in old Scottish.

One of the strangest traces of the old language from which all the Indo-European or Aryan languages were derived, is found in our auxiliary forms, *were*, *wert*, corresponding to the German *werden*; for, going back, we find in Anglo-Saxon *weorthan*, in Old High German *werdan*, in Gothic *vairthan*—that is, we find the root forms *weorth*, *werd*, *vairth*—while in Latin we have *vertere* (root *vert*) and in Sanskrit *vrt* (the exact vowel sound before the *r* unknown).

All this has been brought to my thoughts by the idea of a new universal language—a sort of correction of the Babel mistake, so far as the language of commerce, at least, is concerned. This new language has cost its inventor, M. Schleyer, of Constance, no less than twenty years' labour; but it can be learned in eight lessons (he says). Supposing all nations accept it, and all men engaged in business take their eight lessons, this pleasing language will occupy the same sort of position the world over which "pigeon English" now occupies in China. Its vocabulary is a miscellaneous gathering from many languages, English claiming a goodly share. All its words are monosyllables. Time is *tim* in Volapuk, as the new language is cheerfully called; stone is *ston*, and smoke is *smok*. So far all is easy, we seem to have been speaking Volapuk all the time, without knowing it. But alas, wisdom is *sap* (*verbum sap*); pain is *dol*, and a river is *flum*, all which words are objectionable to English ears. Then the monosyllables are put together into words suggestive of worse than Babel. Thus we are told to believe that *Liko stadols* is a suitable way of saying, "How are you?" and *Glidi sol* a proper expression for "Good day, sir."

Judging from the history of former attempts in the same direction, Volapuk is not likely to come very soon into general use.

It is interesting to observe how language illustrates the past development of races. We may find no material records of the spread of a race over the earth's surface, and yet be as certain from the evidence of language that the race has occupied such and such regions as if we found the remains of buildings they had erected, or of works of various kinds which they had executed. We can in this way find also in language some evidence as to the part of the earth from which the Indo-European races originally spread. It would seem that the Aryan Indians of Lower India came from the Punjaub, and earlier still our Indo-European ancestors appear to have occupied the Highlands of Central Asia to the west of Belurtag and Mustag.

THE GREAT LICK TELESCOPE.*

BY RICHARD A. PROCTOR.

THE time draws near when science will learn how much the great Lick Telescope, perched on its exceeding high "mountain," may be expected to reveal. The expectations of the world outside science are also "exceeding high," especially in America, and still more especially in the far West. Yet we have had some disappointing experiences during the past ten or twelve years, which should lead us to limit our expectations in regard to the achievements of very large telescopes. Those recently made seem to have hitherto proved too much for the observers employed to use them; at any rate, observers very active in discovery when employing smaller telescopes, have done little when set to work with these larger ones, so that grave fears have been aroused lest science may have been the loser, instead of being, as had been hoped, very greatly the gainer, through the construction and erection of these powerful telescopes.

There can be no doubt that theoretically a large telescope is capable of doing better work than a small one. Not only are the space-penetrating and illuminating powers of large telescopes greater, but their defining powers are also better, even when their magnifying powers are not pushed to their full range. A close double star, for example, which an 8-inch telescope shows as two disks touching or overlapping each other, will appear with the same power applied to an 18-inch telescope as two well-defined disks, clearly separated from each other. Increase the power in each telescope, and whereas in the former no separation is effected, with the larger the dark space between the disks is correspondingly widened. Let the smaller telescope even be the most perfect work of Alvan Clarke and his skilfully co-operative sons, while the larger is perhaps the work of only a second-rate optician, yet will the larger attest its superiority in the same decisive way. All the skill even of the Clarkes would not enable an 8 inch telescope to separate double stars lying within a certain degree of proximity which a 15-inch or 18-inch telescope would resolve quite easily. And manifestly, when we consider that every object examined with a telescope consists of multitudes of points whose aggregate

* From the *New York World*.

forms the image of the object, we see that the defining powers as indicated in the study of close double stars, must determine also the comparative powers of telescopes in showing the details of the surfaces of sun, moon, planets, comets, and star-clouds.

There can be no doubt whatever of the potential value of very large telescopes. If we could estimate the value of a telescope simply, without regarding the man at the small end of it (stay: with reflecting telescope we find him sometimes at the large end), we could have no doubt as to the desirability of setting our Clarkes and Grubbs and the rest of them making the largest telescopes money can buy. But if we are to judge by the experience of the last twelve or fifteen years, we must infer that very large telescopes may fail in actual service, even as the great two-handed swords of the Swiss failed in the hands of warriors effectively skilful in the use of less unwieldy weapons. (*Teste* Arthur Phillipson in "Anne of Geierstein.")

The first example which occurs to me is that of an esteemed friend of mine in England. Nothing could have been more admirable than the way in which he worked alike with telescope and spectroscope, and with both combined, when he possessed only an eight-inch telescope. Every one expected that when a convertible fifteen and eighteen-inch telescope was placed at his disposal by the Royal Society he would effect correspondingly enhanced achievements in the way of discovery and research. But all the work done by him with the large telescope during the last twelve years is much more than outweighed by his work in 1864 alone with the small one.

In America examples are more numerous simply because a much greater number of large telescopes has been erected here. (Newall's and Buckingham's large telescopes in England were such utter failures that one is moved by pity not to touch on their achievements—at least, one would be if there were any achievements to touch on.) There was the great telescope at Chicago, an 18-inch of Alvan Clarke's best workmanship. Beyond enabling Burnham to test the value of the double-star observations he had made with a much smaller instrument, this splendid telescope has done nothing. A treatise on astronomy which mentioned nothing discovered with the great Chicago telescope, or with the equally large telescope set up at Charlottesville, near Richmond, would be certainly none the worse for the omission.

One cannot say this of the great telescope at Washington, because no treatise on astronomy would be complete which did not mention the two moons—if moons they should be called—of the planet once called by Tennyson the "moonless Mars." Undoubtedly the discovery of these two bodies was an interesting achievement. Their very minuteness, though detracting from their importance, adds to their interest, and also of course made their discovery more difficult. They may not be moons in the sense in which such orbs as our own moon and the moons of Jupiter are so called; for the two together would probably make but about one-millionth part of our own moon's volume. But they have enabled astronomers to correct their measure of Mars's mass, and they have suggested most instructive thoughts as to the probable processes by which the Martian system was formed. The great telescope at Washington will always be honourably remembered and Professor Hall always gratefully mentioned in connection with the discovery of these tiny bodies. Yet, regarded as a telescopic achievement, this sole work of the great 26-inch telescope (except two or three fairly good drawings of Saturn by Trouvelot, some mediocre pictures of Jupiter and an "atrocity" by Mr. E. S. Holden, claiming to represent the great nebula in Orion) can hardly be esteemed impressive. It would have been very discreditable if the moons of Mars had escaped discovery by an instrument which, it now appears, has ten times the light-gathering power necessary to show them even under much less favourable conditions than existed in August 1877, when they were discovered. True, many very powerful telescopes which ought to have discovered the Martian moons, had been turned on Mars without showing them. Had the Washington telescope failed, as used by Professor Hall, the discredit of the failure would have had to be so widely shared that it would not have amounted to much. True, also, that there is nothing new in the recognition of different objects with small telescopes after they have been discovered by large ones, and when the observer knows just where to look for them. (My friend Mr. Ward, of Belfast, can actually see two of the moons of Uranus with a 4-inch telescope.) But neither circumstance can take away from the fact that both the moons of Mars were easily within range of the great telescope at Washington when discovered, and that they could only be missed through such carelessness of survey as unfortunately had been usual in all former search for Martian satellites.

Probably Professor Hall would be the last to compare his discovery of these two bodies with William Herschel's discovery of two moons of Uranus, remembering how in his own case a splendid telescope had been constructed for the work by the ablest opticians living, whereas Herschel made his own telescope, while also—a circumstance not dwelt on quite so much, I think, as it should have been—Herschel had not only made the telescope with which he discovered the moons of a remote planet, but had begun the work by first discovering the planet itself on which those moons attend.

I do not care to dwell on the most striking example (in America) of the disappointment apt to result from the construction of a very large telescope for use by an astronomer who has achieved important success with a small one. It has been my special pleasure to dwell on the successes of American astronomers. These even now do not receive nearly the amount of attention in the old country that they merit, and are often treated most unfairly, as in the case of the admirable work of the astronomer whose loss science still feels—my valued friend, the late Dr. Henry Draper. American astronomical researches were scarcely noticed at all in England till I directed special attention to them, insomuch that an eminent English man of science, somewhat exaggerating, once spoke of me as having "discovered American astronomy" for my fellow-countrymen. It would therefore not be agreeable to me to dwell on disappointments in a case where my own expectations and the hopes of American men of science had been highest. But it is certain that what I have said of our ablest telespectroscopist in England may be matched—and I fear more than matched—by what might be said of the ablest telespectroscopist in America. With a telescope of tenfold greater power he has achieved less in the last ten years* than in any one of the ten years or so preceding them.

When I am asked, then, what science may hope or expect from the great telescope of the Lick Observatory, I feel constrained to reply that, judging from recent experiences, we can hardly expect aught but disappointment, though we may hope ("hope springs eternal in the human breast") for better things. Let science comfort herself with the thought that at least no Young or Huggins will be expended vainly, judging from present appearances, in the attempt to make the big telescope a great success. This at any rate is something, though it would count for more if some despotic

* The telescope has only been actually at work about six years, it seems.

friend of science could take away from these most able observers their big playthings and send them back to the smaller instruments with which they did such noble work.

THE JAPANESE MAGIC MIRROR.

By J. PARNELL, F.R.A.S.

I THINK I can give Dr. Hutchinson the information he requires, as I investigated the matter more than twenty years ago, and had the good fortune to arrive at a satisfactory result. If Dr. Hutchinson will examine his mirror carefully he will, I think, find that it is not absolutely plane, but more or less convex, and the phenomenon in question is due to the fact that the portion of the surface behind which is a raised figure is, if not plane, at least flatter than the rest of the mirror. Consequently when a parallel beam of light, such as comes from the sun, is reflected by the mirror on to a screen the general convex surface will disperse the light more than the plane portions, which in consequence will appear on the screen as bright figures. I arrived at this result by a careful examination of the distortions produced in the image of a luminous globe as viewed in the mirror, and the result was published in 1866 in *The Reader* (since extinct); and afterwards, in July 1877, finding that the explanation above given was not generally known, I republished in *Nature* the substance of the previous communication. Subsequently, and I presume independently, in December 1878, Messrs. Ayrton and Perry arrived at the same result, and read a paper thereon at the Royal Society.

CRICKET AND BASE-BALL.*

By RICHARD A. PROCTOR.

FEW base-ball players know much about cricket; few cricketers know much about base-ball. I am not one of those who know much about both games; yet I know more about base-ball than most cricketers do. I have watched many games at it, and three of my sons have played at it during two or three years, in such sort as to be able to explain the points of the games I have watched and to tell me of a number of details which otherwise I should have known nothing about. In regard to cricket I have always been an enthusiast, and until this year, when the last available spot at St. Joseph was built over, I have played at the game whenever I had the chance. On the whole, it seems to me I am rather better qualified than most persons to talk about the two games, especially about those points of comparison on which I am about chiefly to touch, which depend on scientific principles of which few professional cricketers or base-ball players would be apt to know much. It happens, rather oddly, that during the past September 1886 an article of mine has appeared in the London *Times* on these special points of comparison, an article entitled "The Break at Cricket and the Curve at Base-ball," and I suppose that it was within less than a week of the appearance of this article in London that Key and Buckland, the English cricketers (one a leading batsman, the other a clever bowler), watched a base-ball game at Philadelphia, and subsequently tested the baffling effects of Fothergill's pitching.

That was not, however, by any means the first time that English cricketers had appeared on the base-ball field. A quarter of a century ago, when Parr and Caffyn brought over a first-class English eleven to America, long before the Australians had acquired the skill which recently enabled them to face on fully equal terms the best players in England (for it was Caffyn who, visiting Australia later, stayed there and taught young England to beat his mother). After the Englishmen had won their last cricket victory, they took part in a base-ball game in which, though they did fairly well, they found the Americans far their superiors. I question, however, whether the pitching of a quarter of a century ago would bear comparison with that which is now regarded as essential in first-class base-ball.

At any rate the English cricketers said nothing when they reached home about the various curves, which perplexed Key and Buckland at Philadelphia; and I cannot but think that if they had recognised these curves they would not only have spoken about them, but would have tried to bring them into effective action in bowling at cricket, as Buckland, I understand, thinks of doing.

Cricket then, as now, needed some new devices in the bowling department, seeing that already the bat was beginning to get the better of the ball. Formerly an innings of over 100 runs was thought excellent; but anything under 200 runs in first-class matches is now regarded as unsatisfactory; and we hear from time to time in matches not far from first-class, of a single member of an eleven at cricket making more than 400 runs off his own bat. (Such numbers as these serve of themselves to suggest to those who know only of base-ball and nothing about cricket, that the two games must be entirely different in character, as indeed they are.)

A few remarks on the essential features of cricket will not be out of place, then, in such an article as this, appearing in St. Louis, where I am given to understand they do not need any particular information about the game of base-ball—though I shall presently have a point or two to notice about base-ball which may be new to some even in the home of the champions of the world.

Cricket is essentially a game of attack and defence. A wicket formed of three upright stumps, with two cross-bars or "bails" at the top, is bowled at by a member of one eleven, while a member of the opposite eleven defends it with a bat. In the game as usually played now, though formerly single-wicket games were common enough, there are two wickets set twenty-two yards apart, each defended by a batsman; and after a series of four balls, called an "over," have been aimed by a bowler at one wicket, another bowler proceeds to trundle four balls at the other wicket. (It is hardly necessary to say that only one ball is used throughout, but the act of bowling is called delivering "a ball.") The bowler, then, attacks while the batsman defends the wicket. If the bowler can hit the wicket, or even touch it so lightly as to bring down one of the bails—the merest graze will generally do this—the batsman is out, and must be replaced by another. If, however, the batsman can strike away the ball in such sort that he and the other bat can exchange wickets before the ball is sent in again, that counts as a run. As many times as they can exchange wickets, each running across from wicket to wicket, so many runs do they count. They may run in this way even though the batsman has not touched the ball, if it is missed by both the wicket-keeper (corresponding in some degree to the catcher at base-ball) and by his backer-up, called the long-stop. Nay, a run is often stolen when the long-stop has not missed the ball, if he is slow in sending it in, and

* From the St. Louis *Globe Democrat* (October 31, 1886). The article is given in full, so that English readers may note whether my description of cricket, for comparison with base-ball, is correct or not.

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the batsman can get across the twenty-two yards in time. Such runs are called byes. But all runs are made at the batsmen's proper peril. If a wicket is put down while the batsman nearest to that wicket is outside his ground (a small space close to the wicket) the batsman is out. So a batsman is out if the ball is caught from his stroke before touching the ground, or if in trying to defend his wicket he strikes it with the bat, even though he may but shake off a bail.

There is yet another way in which a batsman may be put out, and some of the prettiest play at cricket arises in connection with it. The process is called stumping out. In playing at a ball the batsman may get outside his ground; should the wicket-keeper in such a case receive the ball and with it knock off a bail before the batsman can put his foot or ground his bat inside the line which marks his ground, the batsman is out. A good wicket-keeper is as valuable at cricket as a good catcher at base-ball. The feats occasionally accomplished behind the wicket are little short of marvellous. To give an evidence of the neatness with which stumping is sometimes effected, take the following case: (it must be understood that nothing but the sharpest action will do the trick at all, because the moment the batsman finds the ball has passed him he puts his bat down within his ground like a shot). On one occasion, Box, a famous wicket-keeper of the generation before last, took a ball which had passed the bat, hit up a bail with it, caught the bail in the other hand, replaced it, and left it, all so sharply that the first intimation the batsman had of his fate was that conveyed by the umpire's reply, "Out," to the customary query from Box, "How's that, umpire?" All had been accomplished in the mere instant of time following the batsman's failure to hit the ball, and not only before he had time to put his bat down within his ground, but before he could even look round.

What I have said is about all that is necessary to enable one who has never seen a game at cricket to understand the nature of the game. The bowlers attack the two wickets alternately; the batsmen defend and endeavour to make as many runs as they can; the wicket-keeper and long-stops stand behind the attacked wicket, and the remaining players on the attacking side are placed at various points to catch the ball if possible,* or to intercept it, or (failing either) to go after it, and return it *quam celerrime*. As batsman after batsman succumbs, the place of the fallen is taken by another, till at last ten of the eleven have been put out, when, since it takes two players to defend the pair of wickets, the eleven are all out, and their opponents in turn take their innings. Two innings, neither more nor less, on both sides constitute a game, though, of course, both innings on one side may be beaten by a single innings on the other, in which case only three are played.

And here I take it is the weak point of cricket. A great match is always arranged for three days' play; but on the one hand three days may not suffice for the game to be played out, in which case it ends in a draw, even though one side has manifestly the worst of it, or, on the other hand, the game may be finished in two days, or even in one, in which case the balance of the time assigned to the match is lost. To give an idea of the annoyance often occasioned in one or other of these ways, I note that of forty games played by the Australian eleven in England last season, seventeen only were played to a finish (several of these lasting only two days), no less than twenty-three being drawn. (Of the seventeen games, the Australians won nine and lost eight, but as the two leading English counties lost only three games and five games, respectively, of those they played during the season, it will be understood that the Australians were by no means so successful last season as they have been aforetime.) I long since proposed a very simple and effective remedy for this serious fault, from which base-ball is altogether free. I suggested that instead of playing out a whole side at each innings, the sides should go in alternately as man fell after man alternately on either side. In this way the contest could be continued throughout the whole time appointed for the match, with the certainty that neither would any game end in a draw (except in the natural way of equal play) nor would any time be cut to waste in any match.

Cricket being thus different in its very essence from base-ball, which is not a game of defence, all the details are naturally different in character. The very shape of the bats used in the two games indicates the difference between them. The cricket-bat is essentially a guarding bat; the base-ball bat, though most ludicrously pictured in Harper's edition of Charles Read's "Hard Cash" in the hands of a cricketer, and held nearly as a cricketer holds his bat when waiting for the attack, is altogether unsuited for defence. In like manner the cricket-bat would be utterly unsuited for such strokes as the base-ball player requires to make. One of the funniest things to a cricketer's eye is to see a base-ball player striking with a cricket-bat; but, I imagine, it must be at least as funny to a base-ball player to see a cricketer, even one of the most expert, handling a base-ball club. *Usum non habeo*, both one and the other might say with David in the Vulgate (imagine Latin as the vulgar tongue); but either might say more. The kind of stroke given in base-ball would be the worst possible form at cricket; so much I know, and I can guess pretty well what a base-ball player would think of the attitude in which a cricketer is expected to await the attack. "Play with an upright bat," says our rule, and "keep the left shoulder well forward"—how would that look at base-ball?

I am often asked (generally by cricketers) in which of the two games, cricket and base-ball, there is the most science. Nothing, I apprehend, but practical and long-continued experience in both games would enable any one to reply to that question; and very few have had that double experience. The cricketer who knows of the different forms of stroke by which the various devices of the bowler can be defeated and the fielders eluded, who recognises the beauty of the "cut" and the "draw," the attractiveness of the "forward drive" to "on" or to "off," and of the swiping "stroke to leg," the neatness of the "snick through the slips," the quaintness of the "stroke under the leg" (now seldom seen, by the way), when, looking on at a game of base-ball, he sees forward strokes within a limited angle to right and left, alone effective for base making, naturally imagines that there is much less science in base-ball hitting. The base-ball player, on the other hand, noting what seems to him the cramped position of the batsman at cricket, the frequency of "blocking" (that is, merely stopping the ball, or putting it away for safety), especially when the finest players are at work, is equally apt to conclude that cricket is a very inferior game. I remember watching the last three-quarters of an hour of the second day's play between

* There was a song about cricket, in which I often took part, in the good old days of Hullah, wherein we all chorused lustily (Hullahing, so to speak)—

The-en run, boys, run!
Sta-art ev'ry one,
To-o catch the ball
Befo-o-ore it fall!
He's out. The game is e-e-e-ended,
And we the game have wo-o-o-o-on,
And we the game have won.

But the captain of a team of fielders does not, as a general rule, insist on *every* one running to catch the ball; even two would be one too many.

the Australian eleven and Nottingham, our crack cricketing county, in 1884, when the most perfect display of all-round cricketing science was afforded that has perhaps been ever witnessed; and during the whole of that time not only were no big hits made—very likely that would happen in a base-ball match—but no player was got out, no progress made on either side. Every ball was sent down so dead on the wicket, so craftily pitched, so deftly twisted, that the batsmen dared take no liberties; every stroke was so skilfully directed, so nicely timed, so well adjusted, that the fielders got no chances. It was the perfection of cricket, but—it was a trifle monotonous.

If it is difficult to compare the batting science in the two games, it is almost impossible to weigh the relative merits of bowling at cricket and pitching at base-ball. Here, indeed, neither side can doubt the scientific nature of the art. The cricketer, though he never bowls curves, can see that the curve is a beautiful feature of base-ball pitching; and the pitcher, though he never sends a ball which touches ground between himself and the batsman, recognises that in a game where this is not only allowed, but essential, there is room for as much science as in billiards, where all the peculiarities in the motion of the ball arise from contact with the cloth.

The three games may, indeed, in this respect be ranged in order, though the science of all three be nearly equal. The base-ball does not touch ground at all, and none of the complexities of movement resulting from contact with a solid surface affect its motion; but the curving derived from contact with air is a most delicate and difficult scientific problem. The cricket-ball touches the turf once only (in good bowling) on its way to the batsman; but a delicate problem in dynamics is involved in the consideration of the effect arising from the momentary contact of a twisting ball with the ground. And lastly, in billiards, the ball touches the cloth all the time, with effects arising from the constant variation of velocity alike of advanced rotation, which suggest the most delicate and difficult dynamical problems. Possibly cricket, in its intermediate position, supplies the finest opportunities of all three games; for the bowler might cause the ball to curve in the air as well as to twist from the ground; and this, indeed, is what Buckland, the English amateur bowler, is said to intend to try next season. But as yet certainly nothing has been done in this way. It will be a matter of curious interest to note whether anything comes from the lessons in curving given to an English bowler of great skill at Philadelphia last month, by a crack American pitcher. (I trust, by the way, this word "crack" is in use in America, as with us, to mean first-rate. I beg the compositor not to set it crank.)

The curving in base-ball is a more difficult problem to deal with scientifically than twisting at cricket. It is much less easy to make experiments in curving. The effect of twisting is very easily tested, even in the length of a room; but for the curve such sharp propulsion (as well as rotation) is required that experiments can only be made in the field. The problem is one of the prettiest hydrodynamical problems known. (I am quite aware that the word hydrodynamical must here seem entirely out of place; but in our treatises on hydrodynamics, fluids as well as liquids are considered, and the student who wishes to deal with the curving problem scientifically must turn to treatises on hydrodynamics for the necessary formulas.) Every ball flung through the air is in greater or less degree deflected from the course it would have in a vacuum, whether it spins or not. The parabolic path of a projectile is a myth so far as projectiles in air are concerned; and an artillerist who, knowing the velocity of the ball at the cannon's mouth and knowing the angle at which the cannon is inclined, were to calculate the place where the ball would fall on the ordinary parabolic idea, would be out not only by a few yards, but by hundreds, in some cases by thousands. A base-ball is pitched sharply enough to deviate measurably from the parabola without any spin at all. It carries in front of it a little cone of compressed air, which is all the time opposing its advance with an action akin to that of a spring.

But this effect is one which the batsman would take into account as unconsciously, and therefore as uniformly, as a man does who catches a ball pitched to him from a long distance and really travelling yards from the parabolic path; for the slowing of the ball on account of atmospheric resistance takes place in a uniform way. When, however, the ball is pitched with a spin which carries one side of its advancing face forward and the other back, the cone of compressed air in front is no longer symmetrically adjusted. The compression is greater on the side carried forward by the spin, and less on the side carried backward; for on one side the air is not able to slip past the ball so readily as on the other, and naturally gets compressed on that side when its passage is impeded by the friction of the spinning ball.

Now, a cushion of air, though not so hard as the ground or as a billiard cushion, has resisting power all the same. The ball, resisted on that side, is deflected to the other. An odd thing in seeming, though the mathematical equations explain it, is that the deflection does not begin from the beginning or increase gradually, but nearly all of it seems to take place after the ball has passed a certain distance, varying according to the initial velocity. It is this, indeed, which makes the curve so effective in deluding the batsman. The twist at cricket, as at present chiefly used, has no tendency to cause the ball to curve in the air. One side or other of the ball is sharply brought down, so that the axis of rotation lies in the ball's track, and the ball begins, as it were, to roll the moment it touches ground, so that though the touch is instantaneous only, the ball is sensibly deflected.

Gossip.

By RICHARD A. PROCTOR.

THE article on large telescopes quoted in this month's KNOWLEDGE from the *New York World* is well meant, as readers will notice, and I think most English students of science, at any rate, will admit that, while I have been generous, to say the least, towards American astronomers ever since I wrote about astronomy at all, nothing in that article is inconsistent with what I have elsewhere written. Yet, somehow, the publication of this article in the widely-circulating columns of the *World* has brought down upon me a small cyclone of mixed reproaches and misrepresentation.

* * *

FIRST, Professor Young, who assuredly has had small reason to complain of anything I have said of his work in the past, complains bitterly now because I have suggested that the kind of work he is doing with the very fine telescope placed in his hands at Princeton College, N.J. (expressly, be it understood, that he should go ahead of his former self, as well as of his great English rival, Mr. Huggins), is of very little real value. True he has done work which he could have done with no smaller telescope—he has measured positions of Phobos and Deimos, those two faint satellites of Mars, and (I believe) of Hyperion, the seventh and most difficult satellite of Saturn, while a number of close double stars and very minute companions have had their distances and position-angles measured by him, or under his superintendence. But when the value of all this towards the

advancement of astronomy is inquired into, neither he nor anyone else can give any satisfactory answer; while, for my own part, I say very confidently, "These things you should have done"—they *have* a sort of value—but "you should not have left the others undone." And when I am gravely told that, while as yet the spectroscopic analysis of solar phenomena was new, everything of real interest was done with the small telescopes which Young and Huggins then used, and that in *such* work their large telescopes could make no effective advances, I have no answer but a smile—which is not altogether one of amusement. Mr. Huggins, indeed, *has* done other and good work, though unfortunately led off on a false scent by the hope of photographing the corona.

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NEXT, Professor Asaph Hall writes to me that a friend of his at Washington told him my article denounced him in "scathing" terms, so that he "was quite surprised to find on reading it that, on the whole, it was fair." Yet, none the less, he writes me a letter worded precisely as though, instead of speaking of him in terms of high compliment, I had indeed denounced him. And after entirely mis-reading the article—as if I wrote expecting the discovery of other bodies like the satellites of Jupiter and other planets like Uranus—he asks me why, if the discovery of the satellites of Mars with the large telescope at Washington were so easy, "your friend Holden, who made drawings"—and very bad ones they were—"of Mars all through the opposition of 1875, failed to discover them?"—which will be seen, on a reading of what I say, to be *nihil ad rem*.

* * *

LASTLY, though I might mention a number of other American astronomers who have kicked at my well-meant flicking—being indeed more sensitive than our stolid English workers could well conceive—does not "my friend Holden," as Professor Hall very naturally calls him (for when he introduced Hall and myself to each other at Washington he professed suspiciously warm friendship) renew the friendly ways of 1874 by carefully explaining at San Francisco just what my article itself states about the real superiority of large telescopes, and going on to say that only for a few "cranks" like the Wigginses and Vernors can there be any doubt on such points? In other words, with characteristic "candour," he conveys to his audience, as well as he knows how, that I am utterly ignorant of that which I have myself stated in the clearest terms.

* * *

THE "friendliness of 1874" to which I refer, consisted in the deliberate but anonymous statement in the *Atlantic Monthly*, that my first book, "Saturn and its System" (by which I lost what the profane would call "a pot of money," as Messrs. Longmans know) was so profitable, and "deservedly," he was good enough to say, that I was tempted to write a number of other books in order to make a large sum of money; and he went on to denounce among other books, by name, a book which he could not possibly have read, since it was at that time (though announced for completion earlier) lying half written only in my desk. Quite unaware of the authorship of this stupendously unveracious critique, I denounced the unknown writer in very plain terms in a letter which I sent to the chief American newspapers, several of which scathingly denounced his conduct in articles bearing such headings as "Dishonest Criticism," "Spiteful Reviewing," and so forth. The unknown critic replied, admitting his wrongdoing, but defending it in terms suggesting that he really was not aware of its iniquity. And then, still unaware of his identity, I simply declined to accept the apology, just as any good citizen would decline to accept the apology of a pickpocket, who, caught in the act of abstracting a purse, explained that he really had mistaken that purse for some one else's, and begged to apologise, &c., &c. The letter will be found in the *English Mechanic* for December (11th, I think), 1874.

* * *

ALAS! what was I to do, when I found, in 1876, who the offender really was? I know what I did. I remained silent, deadly ashamed and sorry for many a long year. But Professor Holden has brought the matter back to my recollection by virtually renewing his offence. Nay, he has deliberately repeated the untruth about my "Saturn," in company with several other new untruths, which it were weariness to specify.

* * *

HE pretends to believe that I am angry with him because he does not approve so much of my popular writing, which has brought me a maintenance, as he pretends to approve of my scientific writing, which might almost be said to have cost me one. His criticism could not hurt my feelings, since it applies to readers and buyers of my books rather than to me. If the public is interested by sound but simple exposition of scientific truths, and declines to be interested in such matter as I dealt with in "Saturn," the first edition of "The Moon," my "Geometry of the Cycloids," and the like, the fault is not mine, if fault there is at all. If I have made a decent and, I think, an honest livelihood by the explanation of scientific matters for unscientific folk, I have had this to justify me, that in no other way could I have continued my scientific *work*. I had the choice between professional but altogether unscientific work, official scientific work (wanting in independence—and perhaps requiring a little jobbery—and not leaving me free for original research), and just such work—explanation of matters scientific in lectures and books—as I have actually done, not wholly without success. By selecting the last course I have been able to carry on my original work, as I hope shortly to show in a treatise on General Astronomy, which will appear (in its first monthly part) next October. Most certainly I find nothing to be concerned at, when a man who has received large annual salaries at Washington and elsewhere for failing to do satisfactory work with magnificent opportunities, chooses to complain that my explanatory books have not been so unprofitable in the pecuniary sense as the more difficult treatises have invariably been.

* * *

WHAT has really concerned Professor Holden has been my being selected by the editor of the "American Cyclopædia" in 1871–75 to write the astronomy and meteorology for that book—work which he probably thought would have better suited him.

* * *

Map VI. of the "One-Scale Atlas" will appear in the August number, being crowded out by other maps this month.

Reviews.

Palæolithic Man in N.-W. Middlesex. By JOHN ALLEN BROWN. (Macmillan & Co.)—From the standpoint of prehistoric archæology there is very little, if anything, new to be added to our knowledge of man of the Drift period in Britain. Nevertheless, every item of corroborative evidence is welcome, and Mr. Allen Brown has done useful work in exploiting the gravels and brick-earths of the Ealing district in search of the stone implements which are the universal witnesses to man's presence and primitive low culture.

While the interest of his book is primarily local, it will be found serviceable as presenting in compendious form the materials of our knowledge of palæolithic man gathered since M. Boucher des Perthes' renowned discoveries in the Somme Valley. The lithographs of stone tools and weapons in use among existing savages enable the reader to deduce what was the condition of the races which have left like relics in N.-W. Europe.

The Ancient Cities of the New World.—By DÉSIRÉ CHARNAY. Translated by J. Sonino and Helen S. Conant. (Chapman and Hall.)—The glamour which long hung around the ruined cities of Central America, their vast and highly-ornamented palaces and temples, their pyramids and statues, and the great dynasties of the Aztecs and Incas, all invested with a mythic antiquity, has faded away before the "dry light" thrown by sober exploration and common-sense interpretation. The Marquis de Nadaillac's "Prehistoric America" did good service in this direction, and although M. Charnay ventilates questionable theories of "origins" in this book, he admits their small importance. His explorations, the means for which were supplied by France and a wealthy American (Mr. Lorillard), range over thirty years, and form an extremely interesting though somewhat entangled record, through M. Charnay mixing up his adventures as a traveller with his work as an explorer. Following earlier theorists, he contends that the American civilisations, the relics of which are described and amply illustrated, are comparatively modern, and due to a pre-Aztec people, the Toltecs, who founded their empire in Mexico, and, after its disruption, transmitted their industries and mechanical arts to the people who succeeded them. Comparing the architectural remains and the evidences as to the modes of life with analogous materials scattered over the East, M. Charnay inclines to the theory of Toltec migration from Asia. But this convenient reference of all arts and civilisation to Oriental origin is somewhat played out: the generally uniform mode of development of races that have emerged from barbarism is fatal to precise theories of localisation of origin, and we may dismiss the question by advising the reader to study and form his own conclusions from the facts which M. Charnay has narrated in a style that appears to have lost nothing in its transfer from French to English.

The Western Avernus. By MORLEY ROBERTS. (Smith, Elder, & Co.)—We fear that this remarkably interesting book, with its intense humanness, pathos, brightness and beauty of style, is in danger of being overlooked in the crowd of ephemeral stuff that cumbers the reviewer's table and the circulating libraries' counters. The author puzzles us. Here is a refined and highly educated man, who with dyspepsia in his body, Emerson, Virgil, and Carlyle in his wallet, and apparently only enough money in his purse for the voyage, makes tracks from London to Texas, then to British Columbia, and finally to California, hiring himself out as bull-puncher, navvy, dock labourer, miller—in short, for any job to keep body and soul together; makes himself at home by his cheery tact and good humour, whatever his company; tramps weary miles in search of work, sleeps in railway trucks, and, after two years of roughing it, returns to England, to find, we earnestly hope, a large circle of appreciative and grateful readers of his adventurous and thoughtful narrative.

Arcady: For Better for Worse. By AUGUSTUS JESSOPP, D.D. (Fisher Unwin.)—The earnest and sympathetic essays which compose these sketches of the life and general condition of the East Anglian peasantry well deserve rescue from the oblivion of the magazine in which they appeared. The Arcady which the learned author describes with shrewd insight, humour, and pathos, is no ideal state. The picture is charged with shadows; yet, on the whole, brighter than one might expect in these days of low prices, vacant farms, and general rush of the more able-bodied and intelligent villagers to the towns. The book raises and discusses temperately many economic problems, and to the treatment of every topic Dr. Jessopp brings a well-stored and sympathetic mind, which should secure his book serious attention from social reformers.

A Manual of our Mother Tongue. By H. MARMADUKE HEWITT, M.A., LL.M., late Assistant Examiner in the University of London, &c., &c. (London: Joseph Hughes. 1887. Price 7s. 6d.)—This work, one of "Hughes's Matriculation Manuals," we cordially welcome. It seems to us to supply a want—a very different thing from being written to meet one, as so many school books profess to be. We know of no other book that we could so cordially recommend to students preparing for the London Examination. The subject as treated by the examiners has always been wide, and the syllabus vague; so that the numerous candidates have been compelled to read widely to cover the ground, with serious results to the depth of their knowledge. The work is evidently the product of a practised teacher. Where he has thought compilation better than original matter or treatment, large quotations are given and acknowledged. Alford, Abbott, Cobbett, Angus, Mason, Trench, and others are authorities behind whom there is no need to go, and their lucid exposition of principles is admirably made use of by the author in their appropriate place. The book is well arranged. Different type is used to accentuate important lists, divisions, sections, &c., &c., and at the end of the book are the examination-papers for the last ten years set at London University Matriculation. Blank pages are bound in for additional notes; and specially to be commended are a large number of Answered Questions upon various portions of the work. These may, and doubtless will, be taken as models by the student with advantage. We think the book will have a good sale.

Life of Darwin. By G. T. BETTANY. (Walter Scott.)—There is nothing new in this brief sketch of the great naturalist's life and work. The man himself is not brought vividly before us, and the book reads more like a magazine article than a biography. But it has an especially valuable feature in the bibliography of Darwinism prepared by Mr. Anderson, of the British Museum. Mr. Bettany's proof-sheets have been looked over by Mr. Romanes, who therefore endorses the author's description of him as "Darwin's prominent disciple," and as "an undoubted authority in mental science."

Notes of a Naturalist in South America. By JOHN BALL, F.R.S. (Kegan Paul, Trench, & Co.).—The modesty and thoroughness of this book are what might be expected from the cultured observer who gives us the record of his five months' tour round a continent where one sees the Southern Cross standing high in the firmament, while its scenery, north of Terra del Fuego, with its snow-covered peaks and huge glaciers, blended with foreground of dense forest and luxuriant vegetation, is described by Mr. Ball as unlike anything else in this world. Apart from much that is of special interest to the naturalist, the book adds to our knowledge of the social and political condition of the different countries visited, and has practical as well as scientific value for those whom Mr. Ball's pleasant experiences may induce to undertake the journey.

Through Masai Land. By JOSEPH THOMSON, F.R.G.S. (Sampson Low & Co.)—Whilst all the world is reading Mr. Rider Haggard's African fictions, this new edition of Mr. Thomson's African facts, as to which no "strange case" can arise, is opportune. Young as Mr. Thomson happily still

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is, the rough apprenticeship which he had served in explorations of the great continent led to his selection as head of the Geographical Society's expedition to the land of the ferocious Masai, and well did he justify the choice. The story is one of unflagging interest and excitement, told with manly simplicity, and with touches of humour which once more belie Sydney Smith's charge of its absence in Scotchmen. How Mr. Thomson, at starting, could hire only a scratch crew, made up of the scum and riff-raff of Zanzibar, which he converted by his firmness and tact into loyal and decent men; how that same tact carried him along his perilous route, save when buffaloes gored and tossed him, and fever brought him to death's door; how he had to play the part of medicine-man, and with the aid of saliva, false teeth, Eno's fruit salt, and theodolite, work wonders in bringing rain, healing cattle, and other marvels of a character which we must leave our readers to learn for themselves, are all told in a book brimful of good things for the general reader, and of valuable notes on the manners, customs, and vague beliefs of the Masai for the anthropologist.

Social History of the Races of Mankind. Papuo-and Malayo-Melanesians. By A. FEATHERMAN. (Trübner & Co.)—This closely-packed volume is as difficult to review as an encyclopædia. Dealing with the physical environment and character, the modes of life, dress or the absence of it, enjoyment, business, food, dwellings, government, language, birth, wedding and death customs, superstitions, legends (though too sparsely), and religions of two branches of the great Melanesian stock, embracing many grades of culture, from the savage Papuan to the more important and advanced people of Malay proper, it is essentially a working and, on the whole, an excellent *vade mecum* for the sociologist. In the enormous mass of detail, gathered and deftly woven together from numerous authorities duly scheduled at the end of each section, no microscope is needed to see that the author does not always seem awake to the significance of isolated facts; but we wonder that no reference should be made to the totem-signs and names of various members of the groups as indicative of belief in animal ancestry. Mr. Featherman is ill-advised in giving some reviewer of his former book occasion to blaspheme by the long preface which has only indirect relation to the subject-matter of the volume. We respect his independent exercise of judgment in treating of the intricate problem of geographical distribution, but his arguments in support of various originating centres ignore much that has come to light in further support of Mr. Wallace's theory since its first enunciation.

The Geology of England and Wales. By H. B. WOODWARD. (G. Philip & Son.)—This new edition of Mr. Woodward's work is wanted. The book has deservedly taken its place as the standard authority on English and Welsh geology, especially for practical application by engineers and others engaged in mechanical industries by virtue of its ample references to the principal localities where the several formations may be studied, and its consequent interpretation of the maps of the Geological Survey. It is copiously illustrated with figures of sections and accompanied by a good geological map in loose pocket.

The Blood Covenant: a Primitive Rite and its Bearings on Scripture. By H. CLAY TRUMBULL, D.D. (George Redway.)—This book is by an American divine who starts with the intention, which of course is fulfilled, to make the facts square with certain Christian dogmas. He has collected from various ancient and modern sources a number of interesting references to the widespread rite of inter-transfusion or mutual tasting of blood as imparting a sacred and binding character to covenants between individuals or tribes, and it is not difficult to see how the early identification of blood with life would lead to its symbolical use in the relations of man with gods and with his fellow-man. But of course so simple an explanation suffices not for theologians, who regard all barbaric rites and ceremonies as distorted vestiges of a pure original revelation, and hence Dr. Trumbull sees in the blood-covenant an obscured antetype of the communion which Jesus has established with all believers in the drinking of his blood, symbolised by the wine of the Eucharist. Except for its facts, therefore, which are honestly cited, the book is valueless.

The Metal Turner's Handybook. By PAUL N. HASLUCK, A.I.M.E. (London: Crosby Lockwood & Co. 1887.)—The reputation gained by Mr. Hasluck by his now familiar book, "Lathe Work," is sustained in the smaller volume before us, in which within the compass of 140 pages he contrives to impart a very considerable amount of information on the subject of metal-turning. In his description of the various types of lathe and accessory apparatus, he appends in each case the price at which the things described are purchaseable; but as he ranges over the forms constructed by a considerable number of makers, this innovation can scarcely be said to assume the form of advertising. Certainly it will be found useful by the beginner possessing a definite knowledge of his own pecuniary means, but hazy and uncertain as to what is procurable with them. The illustrations are numerous and excellent, there is a good index, and, in short, the beginner in metal-turning will find this a trustworthy guide to a recreation at once serviceable and pleasant.

Notebook on Plane Geometrical Drawing. By ROBERT HARRIS. (London: Hamilton, Adams, & Co. 1886.)—This work may be confidently recommended to all who wish to master the technicalities of geometrical drawing; its author's obviously intimate personal acquaintance with his subject enabling him to simplify his explanatory text so as to render it easily apprehensible by the beginner. His chapter "Concerning Scales" is particularly good; while that on "Graphic Arithmetic and Statics" will open a wholly new world of investigation to a good many of his readers. But why will he say (p. 109) "The double accent (″) signifies inches"? It does *not* signify inches, any more than "£" or "cwt." signify inches. It signifies seconds of arc and nothing in the world else; and every one who has the purity and definite connotation of scientific terminology at heart is bound to protest with all his might against the perpetuation of such a solecism as is contained in the sentence we have quoted.

Iron Bridges of Moderate Span, their Construction and Erection. By HAMILTON WELDON PENDRED. (London: Crosby Lockwood & Co. 1887.)—Intended as an instruction book for the practical constructor of iron bridges of some 140 or 150 feet span, we can only regard Mr. Pendred's little volume as the very model of what such a work should be. The formulæ more or less recondite with which modern works on engineering are so largely occupied find no place in his pages, and the working student who opens them in fear that they will be found to swarm and pullulate with equations for the estimation of thrusts and strains will find to his delight that nothing of the sort appears between its two covers. Written by a man quite obviously personally familiar with the minutest details of the operations and materials he describes, this book contains just the kind and amount of information needed to enable its readers themselves to repeat what is so very clearly explained. The work is a credit to "Weale's Series." If we knew of any higher praise, we would bestow it.

Moffatt's Deductions from Euclid. (London: Moffatt & Paige.)—This book consists of no less than 615 "Deductions" from, or supplementary propositions to, those to be found in the first six books of Euclid, and may be heartily commended to all who wish to familiarise themselves with the leading facts of geometry as demonstrated by the Euclidean method. The careful student of the work under notice ought to be able to answer any and every catch question or "rider" likely to be set in an examination in the immortal book to which they form a valuable addendum.

Practical Electricity. By W. E. AYRTON, F.R.S. (London: Cassell & Co. 1887.)—As a thoroughly sound and trustworthy introduction to the science of Quantitative Electricity, it would be difficult to improve upon Professor Ayrton's latest work, containing, as it does, the fullest description both of the theory and practical construction of the instruments employed in electrical measurements. But the reader who goes to its pages for information as to dancing pith-figures, cork spiders, illuminated spiral tubes, and other prettinesses of the whilom lecture table, will be doomed to disappointment; for nothing of the sort appears between its two covers. In its stead he will find a mine of information as to the nature of the electrical current, electro-motive force, potential, &c., and will be able to realise the signification and use of those units of measurement, the ampère, the coulomb, the farad, the ohm, the volt, and the watt. Professor Ayrton's volume is, in short, a reproduction of his first year's course of lectures on electrical engineering delivered to the students at the City and Guilds of London Institute, with the laboratory practice by which they are illustrated. It is a sound and conscientious piece of work.

The Modern Practice of Shipbuilding in Iron and Steel. By SAMUEL P. THEARLE. Vol. I., Text; Vol. II., Plates. (London and Glasgow: William Collins & Co.)—Mr. Thearle has made, in every respect, a worthy addition to the Advanced Science series of Messrs. Collins & Co. in the work now before us. Presumably intended as a text-book for examinees of the Science and Art Department and of the Admiralty, it is really too good for such a purpose, possessing as it does very real merit as a technological hand-book. Its descriptions are exhaustive, and the illustrations leave nothing to be desired. Now that our "hearts of oak" survive only in the chorus of an almost forgotten sailors' song, the practice of iron shipbuilding must possess an interest for all concerned in the maintenance of our maritime supremacy, to say nothing of mercantile shipowners and yachtsmen, who have a special personal concern in it. In fact, all who go down to the sea in ships, or who have to trust their lives to the passenger vessels of the present day, may spend an hour or two very much more unprofitably than in the study of Mr. Thearle's capital treatise and its accompanying atlas. Such study cannot fail to impart intelligent ideas as to the manner in which the floating towns which now connect the most distant parts of the globe are made so staunch, safe, and comfortable as the larger proportion of them undeniably are; for, assuredly, wherever "ignorance is bliss" it is not in the selection of a vessel to make a long voyage in.

Old or New Chemistry. By SAMUEL E. PHILLIPS, F.C.S. (London: Wertheimer, Lea & Co. 1886.)—Mr. Phillips is a man with a grievance. In November 1885 he applied to the Council of the Chemical Society for a pecuniary grant to make numerical analyses of the indium and the gallium alums. In vulgar, but expressive, parlance, the Society "didn't seem to see it." Again in May 1886 he sent a paper to the Society on the Urethane Reaction, only again to meet with a refusal. *Hinc illæ lachrymæ.* In a highly technical series of essays our author exposes (or believes that he exposes) some of the fallacies underlying the new system of chemistry. The very technicality of which we have spoken unfits his little book for popular reading, but it may be commended to such chemists as care to hear both sides of the question.

The Lads of Little Clayton. By R. STEAD. (London: Blackie & Son. 1887.)—This little volume is made up of a series of short detached papers describing, very naturally indeed, the doings and misdoings of the lads of the lower and lower middle classes in an English village some five-and-twenty or thirty years ago. The lads are real flesh-and-blood ones, in many cases rough and even brutal, and their conversation is such as may even now be heard on any village green. Where, as in the majority of cases, any moral is inculcated, it is never obtruded, but springs naturally from the *dénouement.* This would be a first-rate book for a parochial lending library. It is as innocent as it is interesting.

Messrs. Longmans send us *Social Arrows,* by Lord BRABAZON, whose practical efforts to improve the moral condition of our crowded centres by the wholesome material remedies of more open spaces, and to lighten the toil of the overworked shop assistant, have our deep sympathy. *Spirit Workers in the Home Circle,* by MORELL THEOBALD (Fisher Unwin), is illustrated by facsimiles of "direct spirit-writings," and blames the Society for Psychical Research for assuming that it is dealing with phenomena subject to known physical laws. It is, we presume, hopeless after this to commend to Mr. Theobald, with his disregard of Time and Space, Dr. BITHELL'S *Agnostic Problems* (Williams & Norgate), but we may advise the readers of KNOWLEDGE to make acquaintance with it. Of *English as She is Taught* (Fisher Unwin) the cream has already appeared in the *Century Magazine. Edward III. and his Wars* and *The Misrule of Henry III.* (Nutt) are modernised extracts from contemporary chronicles, designed to cover the whole period of Mediæval and Renaissance history. The name of Mr. York Powell as editor is sufficient guarantee of the excellence of the series. We have received new editions of Mr. HERBERT FRY'S excellent guide-book, *London in* 1887, and of Mr. SPENCER'S *Road-Book,* indispensable to the cyclist; Nos. 1–3 of the *Essex Naturalist,* admirably edited, and enriched with a facsimile of Norden's map of Essex, 1594; and *Memoirs of the Imperial University of Japan,* containing some interesting myths and traditions of the Ainos.

Our Whist Column.

By RICHARD A. PROCTOR.

A WHIST PROBLEM. LEAD OF ACE FROM ACE TO FOUR.

THE whist editor of the *Australasian,* in a note on my discussion of the whist superstition that after a misdeal the next deal will almost certainly give a singleton hand, discusses the question of the probability of a singleton suit in any hand taken at random in any deal. "We showed," he says, speaking of a former inquiry, "that the chance that we should find either a singleton or a blank suit is 7 = 20. Mr. Proctor makes the fraction 4 = 13, which is appreciably smaller."

The difference arises from the fact that the chance of a singleton suit appearing is appreciably smaller than the chance that there will be either a singleton suit or a blank suit. It will be remembered that adding up numbers representing one-fourth of the total number of arrangements by which a singleton suit can appear in a hand (as when the suits are divided 5, 4, 3, 1, or 6, 4, 2, 1, or 6, 3, 3, 1, or the like) I obtained the number 48,882,413,450. To give the chance considered by the whist editor of the *Australasian,* we must add to this number one-fourth the number of all those arrangements in which a blank suit appears, but no singleton (because we have

already counted those in which both a blank suit and a singleton appear). Thus we obtain the addition sum:—

Arrangement.	One-fourth of the Number of Hands in which arrangement can appear.
6, 4, 3, 0	2,105,429,040
5, 4, 4, 0	1,973,839,725
5, 5, 3, 0	1,421,164,602
6, 5, 2, 0	1,033,574,256
7, 4, 2, 0	574,207,920
7, 3, 3, 0	421,085,808
8, 3, 2, 0	172,262,376
9, 2, 2, 0	13,050,180
7, 6, 0, 0	8,833,968
8, 5, 0, 0	4,969,107
9, 4, 0, 0	1,533,675
10, 3, 0, 0	245,388
11, 2, 0, 0	18,252
Total	7,730,214,297
Numbers already obtained	48,882,413,450
Total	56,612,627,747
	Multiply by 4
Grand total	226,450,508,988

That is to say, out of the 635,013,559,600 hands which can possibly be held at whist, no less than 226,450,508,988 show either a singleton suit or a blank suit. Thus the chance of any hand taken at random in any deal showing either a blank or a singleton suit is represented by the fraction

$$\frac{226,450,508,988}{635,013,559,600}$$

which differs very little from (it is really slightly greater than) 7-20, the approximation mentioned by the whist editor of the *Australasian*.

In passing, I may remark that I cannot understand a comment made in the *Australasian* upon my approximate estimate that the odds are about 5 to 2 in favour of the appearance of a singleton in one hand at least out of the four distributed at a single deal. The whist editor states that a friend, from the records of an experience extending over five years, had found that the total number of singleton *hands* is about equal to the total number of deals. "We believe, therefore," he proceeds, "that the approximate result which Mr. Proctor has published is under rather than over the mark." My approximate result cannot be tested by counting the number of singleton *hands*; but my exact result relates directly to them. I showed that in 635,013,559,600 *hands*, and therefore for 158,753,389,900 *deals*, there will be 195,529,653,800 hands in which there is at least one singleton, or about sixteen such hands for every thirteen deals. This is above, not below, the result observed in five years' play, unless the "at least" is to be interpreted rather liberally. But a singleton in one or other hand, when singletons are appearing freely, will often escape uncounted.

The article by the *Australasian* whist editor is aimed at the fallacy of supposing that if a player holds Ace and three others (not including the King, or both Queen and Knave) he can safely lead a small one where one trick is necessary to save the game. He wishes me to turn my mathematical deduction to practical account by diverting it from the superstition I attacked to one which affects actual play. For, as he points out, when so long-practised a player as "Pembridge" confidently asserts that the odds are ten to one against "a singleton being on the *tapis* at all," it is evident that very erroneous ideas are held among whist-players on this point, importantly though it should affect whist play.

However, it is not by an inquiry into the average number of singleton hands, or hands with a blank suit, per hands dealt, that the propriety of an Ace lead from Ace to four, under the condition mentioned, can be decided. Taking it for the moment as decided that where one trick saves the game, the question of playing Ace from Ace to four must be decided by the chance of the Ace being ruffed second round, we have in the case of an original first lead a comparatively simple problem—while if other suits have been already opened our problem becomes so complex (having so many varied forms) that no one would care to deal with it. I take, then, the simple problem:—

If A, the original leader, holds four cards in a suit, what is the chance that one or other of the opponents, Y and Z, will be able to ruff the suit second round? apart from the possibility of either getting discards in the suit.

I get the following figures by taking from the table at page 196 of my "How to Play Whist" (which, be it noticed, not only shows how the suits may be distributed in a hand, but how all the cards may be distributed in a suit), the various numbers set opposite the different arrangements mentioned:—

Distribution of cards of a given suit among the four players, one at least holding 4.	One-fourth the number of ways in which the suit can be so distributed.
4, 4, 3, 2	34,213,221,900*
5, 4, 3, 1	20,527,933,140
5, 4, 2, 2	16,795,581,660*
4, 3, 3, 3	16,726,464,040*
6, 4, 2, 1	7,464,702,960
4, 4, 4, 1	4,751,836,375
6, 4, 3, 0	2,105,429,040†
5, 4, 4, 0	1,973,839,725†
7, 4, 1, 1	622,058,580
7, 4, 2, 0	574,207,920†
8, 4, 1, 0	71,775,990†
9, 4, 0, 0	1,533,675†
Total	105,828,585,005
Sum of numbers marked *, cases in which no hand is blank or holds but one card in the suit	67,735,267,600
Sum of numbers of cases in which at least one hand is blank in suit, or holds a singleton	38,093,317,405
Sum of numbers marked †, cases in which at least one hand is blank	4,726,786,350
Sum of numbers of cases in which at least one hand holds but one card	33,438,307,045
Sum of numbers of cases in which *two* hands hold either a singleton or no card in the suit	695,368,245

It will be seen that if we neglect as numerically unimportant the cases in which two hands are void, or hold no more than one card in the suit, we may say that in round numbers there are for 67,700 cases in which the suit will go round twice, 33,400 cases in which it will go round but once, and 4,700 cases in which it will not go round at all. Of these last-named cases, two-thirds, or about 3,130, in which an enemy holds the hand void in the suit, are of course unfavourable to the Ace lead; but they do not affect our judgment, simply because it is certain that the chance of a ruff first round must be risked in any case. (There is, of course, a much greater chance of this happening when Ace is led from Ace to five.) Of the 33,400 cases in which a singleton is held, we may consider at least 400 to be cases where another hand is either void, or holds a singleton, the two hands thus situate being so distributed between *Y*, *B*, and *Z*, that *A B* do not lose through a ruff. This leaves 33,000 cases of a singleton hand in the suit led by *A*, as against 67,700 cases in which the suit will go round twice. Of these 33,000 cases, 22,000 will be cases in which either *Y* or *Z* holds the singleton, and therefore the enemy can ruff the second round of the suit. Thus, by leading a small card *A* will lose the Ace in 22,000 cases out of 67,700 + 33,000 or 100,700, or, roughly, about twice in nine trials, or more nearly, five times in 23.

How far this 2-9th chance of being ruffed second round would justify a player in leading Ace first, to save a game (which might be saved otherwise) at the risk of losing command over the suit, must depend on the details of the hand and game. It is to be remembered that even though *Z*, the fourth player, should be able to ruff, he would not ruff the suit when returned by *B* in every case, only if weak or very strong in trumps. For as *B* would return, generally, a small card, to ruff would be ruffing a doubtful trick.

Our Chess Column.

BY "MEPHISTO."

MATCH BLACKBURNE *v.* ZUKERTORT.

THE result of this match, 5 to 1 in favour of Blackburne, with 8 draws, has rather taken the Chess world by surprise, and must raise the winner in the estimation of those who apprehended that Blackburne never did himself justice when contesting a match. For nearly forty years, that is, ever since Staunton was in his best form, foreign Chess-players were supreme in this country. By this victory, however, Blackburne has avenged his former defeat by Zukertort in 1881, and thereby an Englishman again occupies foremost rank amongst Chess-players. For the time

being, unless the coming international tournament should decide otherwise, Blackburne may fairly claim the championship of Europe. The play in the match has been good, and above the average. Appended are some interesting particulars and positions.

Zukertort again relied upon the irregular opening which did him such good service in the London tournament. For the purpose of instruction, and to show the various defences adopted by Blackburne, we give here the opening moves of five of the games, in which Zukertort as first player adopted the irregular attack against Blackburne, namely in the 2nd, 4th, 6th, 8th, and 10th game.

1.	P to Q4	P to Q4	P to Q4	P to Q4
	P to Q4	P to Q4	P to Q4	P to Q4
2.	Kt to KB3	Kt to KB3	P to K3	P to K3
	P to K3	B to Kt5	Kt to KB3	Kt to KB3
3.	P to K3	QKt to Q2	P to QB4	P to QB4
	Kt to KB3	Kt to Q2	P to B4	P to K3
4.	P to QKt3	P to K4	P to QKt3	Kt to KB3
	P to B4	P to K3	B to B4	P to B4
5.	B to Kt2	B to K2	Kt to QB3	P to QKt3
	Kt to B3	P × P	P to K3	Kt to B3
6.	QKt to Q2	Kt × P	B to Kt2	B to Kt2
	B to Q2	KKt to B3	Kt to B3	B to Q2
7.	B to K2	Q to Q3	Kt to B3	B to K2
	R to B sq	P to B3	R to B sq	R to B sq
8.	Castles	Kt to Kt3	B to K2	Castles
	P × P	B to Q3	BP × P	BP × P
9.	P × P	B to Q2	KP × P	KP × P
	B to Q3	Q to B2	B to Q3	P × P
10.	P to B4	Castles KR	Castles	P × P
	Castles	Castles QR	Castles	B to Q3
11.	P to B5	P to B4	P to B5	QKt to Q2
	B to Kt sq	P to KR4	B to Kt sq	Castles
12.	P to QR3	KR to B sq	P to QR3	R to K sq
	P to KR3	P to B4 (*b*)	Kt to K5	Kt to K2 (*d*)
13.	P to QKt4 (*a*)		P to QKt4 (*c*)	

(*a*) White obtained the better game, but lost through indifferent play.

(*b*) Black obtained an advantage, but the game resulted in a draw.

(*c*) Resulted in a draw.

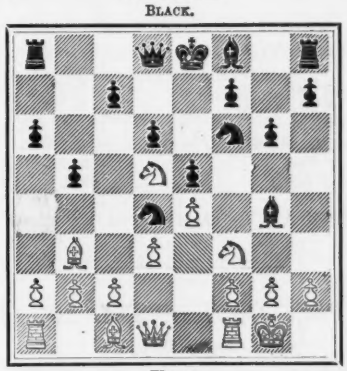
(*d*) Drawn.

The tenth is a very prettily played game, which we therefore give in full.

	WHITE. Zukertort.	BLACK. Blackburne.
1.	P to Q4	P to Q4.
2.	P to K3	Kt to KB3
3.	P to QB4	P to K3
4.	Kt to KB3	P to B4
5.	P to QKt3	Kt to B3
6.	B to Kt2	P × QP
7.	KP × P	P × P
8.	P × P	B to Kt5 (ch.)
	(Inconvenient! White should Castle before playing B to Kt2.)	
9.	QKt to Q2	
	(B to B3 was requisite.)	Kt to K5
10.	Q to R4	
	(Black threatened Kt × Kt followed by Kt × QP.)	Castles
11.	R to Qsq	Q to B3!
	(Very good! Threatening B × Kt (ch.), and the Kt cannot retake. If B to Bsq then Kt × QP wins.)	
12.	B to Q3	
	(He must give up the exchange, there is no other move.)	B × Kt (ch.)
13.	R × B	Kt × R
14.	K × Kt	B to Q2
15.	Q to R3	KR to Ksq
16.	P to R4	P to QR4
17.	K to Qsq	Kt to Kt5
18.	B to Kt sq	P to K4
	(Opening up White's game.)	
19.	P × P	Q to B3
20.	Q to B3	B to K3
21.	P to R3	QR to Qsq(ch)
22.	K to Bsq	
	(Of course he should have played K to Ksq.)	
23.	R to Q6	Resigns
	(For if B × R then Kt to R7 (ch.) wins.)	

Blackburne's favourite opening was the Ruy Lopez. In the seventh game the following interesting combination developed itself early in the game. Blackburne, being White, played 1. P to K4, P to K4; 2. Kt to KB3, Kt to QB3; 3. B to Kt5, P to QR3; 4. B to R4, Kt to B3; 5. P to Q3, P to Q3; 6. Kt to B3, P to KKt3; 7. Castles, P to QKt4 (B to Kt2 is the proper move); 8. B to Kt3, B to Kt5; 9. Kt to K5, Kt to Q5.

ZUKERTORT.
BLACK.



WHITE.
BLACKBURNE.

Blackburne was prepared for this move, and continued with

10. Kt × Kt — 10. B × Q
11. Kt to B6

(A very fine combination, such as does not often occur in match play. Of course, if the Queen moves away, then Kt × Kt mate.)

Kt × Kt

12. Kt × Q — R × Kt

(If instead B or Kt moves, White plays Kt × P.)

13. P × Kt

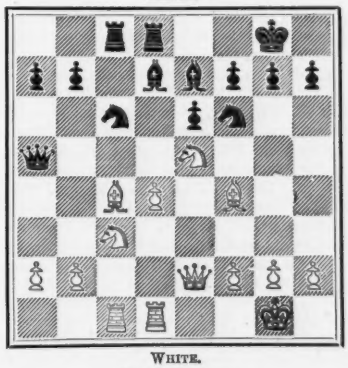
Best as it secures an advantage for White by blocking Black's game, and forming a strong array of Pawns against the weakened Q's side.

B to Kt5

14. P to QR4. (White won).

The following is another exceedingly interesting position, which occurred in the 13th game, where Blackburne played in a brilliant manner, sacrificing a piece for a fine attack.

ZUKERTORT.
BLACK.



WHITE.
BLACKBURNE.

Blackburne played Kt × P! (An elegant move, and perfectly sound.)

K × Kt

P to Q5! — P × P
Kt × P — K to Bsq
Kt × Kt — B × Kt
R to Q5

White might at once have won his piece back by R × B, R × R, Q to K6, R to Ksq, &c., but although White would emerge with a P ahead, the game looked rather drawish, besides the temptation to continue the attack was strong.

P to Q Kt4

A good move, for, if either R or B × P, then Black gains time.

R × P

The position is very rich in possibilities. White might have continued B to Q16 (ch) or R to B3.

Q to R5

XUM

P to Kt3	Q to R6
R to Q5	Kt to Q5
R × Kt	

(Extremely bold play.)

	B × R
R to Qsq	Q to B4
Q to B3	B to B4
P to Kt4	B × BP (ch.)

Black might have forced a win at once by Q to B3.

K to Bsq	B to R5
R × R (ch.)	R × R
P × B	B to B3

K to Kt2. (The game resulted in a draw.)

THE FACE OF THE SKY FOR JULY.

BY F.R.A.S.

SPOTS continue to appear occasionally on the sun, so that the observer should not relax his watch for them. The sun is in apogee on the morning of the 2nd; or, in other words, the earth is at her greatest distance from him. He of course appears smaller then than at any time during the year. The aspect of the night sky is shown in map vii. of "The Stars in their Seasons." Up to July 21 there is no real night in any part of Great Britain. Mercury is an evening star, and at the beginning of the month may be caught with the naked eye over the W.N.W. part of the horizon after sunset. He is in Cancer ("The Stars in their Seasons," map iii.) throughout the month. He is at his greatest eastern elongation from the sun (25° 51′) on the 1st. Venus, as an evening star, is the most brilliant and conspicuous object in the sky. She attains her greatest elongation east (45° 32′) in the evening of the 13th. She exhibits a beautiful crescent in the telescope now. She will be found in the W. by N. after dusk. She travels across Leo ("The Stars in their Seasons," map iii.) during the month. Jupiter is getting so rapidly towards the west, and is so low down, that he must be looked for the moment it is dark enough. He will be found a very little to the north and somewhat to the east of Spica Virginis ("The Stars in their Seasons," map v.). The certainly visible phenomena of his satellites during July number only twelve. They are as follows:—On the 1st Satellite II. will reappear from eclipse at 10h. 5m. 29s. P.M., as will the third Satellite on the 3rd at 10h. 18m. 54s. P.M. On the 6th Satellite I. will be occulted at 11h. 0m. P.M. On the 7th the shadow of Satellite I. will enter on to Jupiter's face at 9h. 33m. P.M., the satellite casting it leaving his opposite limb at 10h. 31m. On the 10th Satellite III. will reappear from occultation at 9h. 29m. P.M. On the 14th the ingress of Satellite I. in transit happens at 10h. 12m. P.M. On the 15th Satellite II. will be occulted at 10h. 11m. P.M., as will Satellite I. at 9h. 17m. P.M. on the 22nd. On the 24th the shadow of Satellite II. will begin its transit over Jupiter's face at 9h. 43m. P.M., Satellite II. itself leaving Jupiter's opposite limb only one minute later! Finally, on the 31st, Satellite I. will reappear from eclipse at 9h. 6m. 43s. Neither Saturn, Uranus, nor Neptune is, for the observer's purpose, visible. The Moon is full at 8h. 34·2m. in the morning on the 5th, enters her last quarter at 6h. 57·1m. A.M. on the 13th, is new at 8h. 50m. P.M. on the 20th, and enters her first quarter at 2h. 30·3m. in the afternoon of the 27th. Six occultations of fixed stars by the Moon will occur at tolerably convenient hours for the amateur observer during July. On the 1st, η Libræ, a star of the 6th magnitude, will disappear at the Moon's dark limb at 9h. 23m. P.M., at an angle from her vertex of 19°; reappearing at her bright limb at 10h. 4m. P.M., at an angle of 320° from her vertex. On the 6th, B.A.C. 7053, a 5½th magnitude star, will disappear at the bright limb of the Moon at 9h. 49m., as will o Capricorni, also of the 5½th magnitude, simultaneously. At the time of their disappearance B.A.C. 7053 will be 37°, and o Capricorni 38° from the vertex of the Moon. The first-named star will reappear at the Moon's dark limb at 10h. 53m. P.M., and the second at 10h. 54m. P.M., both at a vertical angle of 280°. On the 8th, 42 Aquarii, of the 6th magnitude, will disappear at the bright limb 48 minutes after midnight, at a vertical angle of 43°. It will not reappear until about a quarter to 2 o'clock the next morning. On the afternoon of the 17th, an occultation of Aldebaran will happen in bright sunshine, the star disappearing at the bright limb of the Moon at 3h. 16m., at an angle of 139° from her vertex, and reappearing at 3h. 33m. at a vertical angle of 172°, of course at the dark limb. On the 25th, B.A.C. 4277, of the 6th magnitude, will disappear at 8h. 20m. P.M., at the Moon's dark limb, at an angle of 34° from her vertex, reappearing at her bright limb at 8h. 45m., at a vertical angle of 350°. Finally, on the 31st, 21 Sagittarii, of the 5th magnitude, will disappear at the dark limb at 12h. 23m. P.M. The reappearance at the bright limb will not happen until between 1 and 2 o'clock in the early morning of August 1. When our notes begin the Moon is in Libra, but at 5h. 30m. A.M. on the 2nd, she has arrived at the western edge of the narrow northern spike of Scorpio ("The Seasons Pictured," plate xxvi.). By 2 o'clock the same afternoon she has crossed this and emerged in Ophiuchus. Her passage over this constellation occupies her until 6h. A.M. on the 4th, when she enters Sagittarius. She is travelling through Sagittarius until 2h. P.M. on the 6th, when she quits it for Capricornus ("The Seasons Pictured," plate xxi.). Her journey across Capricornus terminates at 4h. P.M. on the 8th, and she then passes into Aquarius. Here she remains until 7h. P.M. on the 10th, when she enters Pisces ("The Seasons Pictured," plate xxii.). From about 7h. P.M. on the 11th to 11h. P.M. on the 12th she is in the confines of Pisces and Cetus, but fairly enters Pisces again at the hour last named. Twenty-four hours later she has crossed again into the northernmost part of Cetus, which she finally quits for Aries at 6h. 30m. P.M. on the 14th. It takes her precisely 24 hours to cross the constellation last named, and at 6h. 30m. P.M. on the 15th she passes into Taurus ("The Seasons Pictured," plate xxiii.). Travelling through Taurus, we find her at 1h. 30m. in the afternoon of the 18th on the confines of the northern part of Orion; 12 hours later she has traversed this and come out in Gemini ("The Seasons Pictured," plate xxiv.). She is travelling through Gemini until 6 P.M. on the 20th, when she enters Cancer; quitting Cancer, in turn, for Leo at 5h. A.M. on the 22nd. She is in Leo until 4h. P.M. on the 24th, at which hour she crosses the boundary into Virgo ("The Seasons Pictured," plate xxv.). It takes her until 3h. P.M. on the 27th to traverse this large constellation, and at that hour she passes into Libra ("The Seasons Pictured," plate xxvi.). Then, as at the beginning of the month, we find her at 11h. A.M. on the 29th on the edge of the narrow northern strip of Scorpio, and this she leaves at 8 o'clock the same evening for Ophiuchus. At 1 o'clock in the afternoon of the 31st she has quitted Ophiuchus for Sagittarius, and is, of course, in Sagittarius when the month closes.

THE London Stereoscopic Company purpose holding their Third Annual International Amateur Photographic Exhibition in October next, when a sum of 50*l.* in cash and a number of gold, silver, and bronze medals will be offered as prizes. Captain W. de W. Abney, F.R.S., as president, and the editors of the *Camera* magazine and of the *Amateur Photographer* have undertaken to act as judges, together with two to be appointed by the plebiscite of the exhibition. The exhibition is under the patronage of several influential amateurs. The whole of the entrance fees will be given in full to the Photographers' Benevolent Association, and we think this idea of the Stereoscopic Company a good one; for while so many wealthy persons are interested in the art, it is well that they should from time to time be reminded of the existence of their less fortunate professional brethren. All particulars relating to the exhibition can be obtained by application to the Company's Secretary, 108 and 110 Regent Street, London, W.

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